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ARTICLES

American Political Science Review Vol. 86, No. 2 June 1992

RETHINKING ALLISON'S MODELS

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The ideas in Graham Allison's Essence of Decision *(1971) have had an enormous impact on the study and teaching of bureaucracy and foreign policy making. While Allison's work has received considerable critical attention, there has been surprisingly little examination of the content and internal logic of his models. We subject each of Allison's three models to a systematic critical analysis. Our conclusion is that the models require substantial reformulation.*

Graham Allison's study of the 1962 Cuban missile crisis, published as "Conceptual Models and the Cuban Missile Crisis" (1969) and as *Essence of Decision: Explaining the Cuban Missile Crisis* (1971), is a landmark in our discipline's analysis of bureaucracy's role in foreign policy making. Prior to Allison's work, most studies of bureaucracy and foreign policy, though rich and informative, were largely descriptive and rather unfocused theoretically. They seldom presented an explicit analytical perspective, and readers were often left to infer what the authors believed were the fundamental properties of bureaucratic policymaking.

Allison's work demonstrated that a more self-consciously theoretical approach to the study of bureaucracy and foreign policy was both feasible and desirable. The earlier literature had rarely developed alternative explanations for events and decisions. But in formulating his three models—the *rational actor, organizational process,* and *governmental politics* models, labeled I, II, and III, respectively—Allison showed how to make explicit the explanations of events that had only been implicit in many of these earlier studies. In so doing, Allison helped to place the study of bureaucracy's influence on foreign policy on a more scientific foundation.

Allison's work received high praise in reviews by a wide range of scholars such as Ole Holsti (1972), Francis Rourke (1972), and Harrison Wagner (1974). For example, as Wagner remarked in his review of *Essence of Decision*:

> It may, in fact, be the most generally persuasive attempt ever made to show the relevance of theorizing to the treatment of what are ordinarily thought to be the main problems of the study of international politics. Its general appeal is primarily due to the effectiveness of Allison's theoretical arguments in his analysis of the Cuban missile crisis, which contains a number of striking insights and some information that no one else had thought to look for. The results are a much more complex analysis of that event than anyone else has offered and a radically different understanding of what happened from the one most people had previously adhered to. To the extent that these considerable virtues are the direct result of Allison's reflections on the theoretical literature mentioned, they seem to be the product of his efforts to make these three paradigms as explicit as possible, enabling him to derive as much insight from the assumptions of each as he could, and to develop alternative explanations for this event, which had the heuristic effect of leading him to look for possibilities that had not occurred to analysts more nearly the prisoners of a single, and poorly articulated, set of assumptions. These are, I think, among the important benefits to be derived from explicit theory construction, and Allison's example is therefore one well worth taking seriously. (pp. 446–47)

As predicted in the reviews, Allison's approach has indeed had a substantial impact on both the study and the teaching of bureaucracy. His three models have been widely applied, and other scholars have followed his lead by developing additional models to account for aspects of policymaking left unexplained by Allison's analyses.[1] Moreover, Allison's book is still widely used in courses on bureaucracy, public administration, and organization theory. We have frequently used it in our own classes; one of us (Hammond) is alone responsible for the sale of several hundred copies to students in the past 15 years. As Holsti's review suggested, the book is so useful because it shows students how to formulate and evaluate alternative explanations for political events (1972, 137). Indeed, for courses dealing with bureaucracy and foreign policy there are few alternatives to *Essence of Decision*. Even now, over 20 years later, the book remains heavily cited by scholars in a remarkably wide range of disciplines.[2]

Allison's work did attract considerable critical attention from the outset (e.g., Art 1973; Ball 1974; Bobrow 1972; Caldwell 1977; Cornford 1974; Freedman 1976; Krasner 1972; Perlmutter 1974; and Yanarella 1975). Even those praising the book (e.g., Holsti 1972; Rourke 1972; and Wagner 1974) also expressed reservations. However, none of these early criticisms comprehensively examined the internal logic of all three of Allison's models, and it is precisely the logical structure of his work that is particularly beginning to show its age.

With the advantage of two decades of hindsight, much more is known about the properties of models of bureaucracy and policymaking. It is now clear that alternative versions of Allison's models could and should be developed to highlight somewhat more fundamental factors in policymaking. Since Allison's work still has wide currency in our discipline, the time is ripe for a critical analysis of his models and arguments.

We make five arguments about Allison's work. First, while a key purpose of modeling is to force the analyst to clarify the assumptions on which the analysis will be based, it is often difficult to determine for Allison's models just what the assumptions are. If one cannot determine what is driving a model, its explanatory power cannot be accurately assessed. Second, the hypotheses drawn from a model should, ideally, be logically derived from the initial assumptions. But many of Allison's hypotheses seem, at best, only loosely related to the models' initial assumptions. Third, whatever the logical status of Allison's propositions (are they rigorously derived from his assumptions, or are they freestanding conjectures?), it is possible to demonstrate, on strictly logical grounds, that several key propositions in Model II are simply incorrect.

Fourth, a well-crafted model must strike a balance between simplicity and complexity. Too simple a model misses key aspects of the problem one is trying to understand; too complex a model is analytically intractable and yields few testable hypotheses. Of course, what is too simple and what is too complex is partly a matter of what analytical technology is available. Nonetheless, even given what was available 20 years ago, we think that it is fair to say that Allison's Model I is (and was) much too simple. On the other hand, we will also argue that Model III is, in some ways, too complex. Finally, Allison's Models I, II, and III are based on three bodies of literature (rational choice theory, organization theory, and bureaucratic politics, respectively) that only specialists in these fields know well. Specialists in other fields, such as foreign policy making, have often used *Essence of Decision* as an introduction and guide to these literatures. Unfortunately, Allison misinterpreted parts of each of these three fields. In some cases, the misinterpretations were, in our judgment, quite substantial. Thus, unsuspecting readers may be led into a double error—believing that the models are better constructed than they are and that they accurately reflect the literatures on which they are based.

We examine the various sets of assumptions on which the models are based. These sets of assumptions will be used to develop a typology of policymaking models, thus providing a context for analyzing the models in *Essence of Decision*. We then probe the logic of the three models and conclude by discussing some further implications of our arguments.

We should emphasize that our general purpose here is to examine the *theoretical* aspects of Allison's work. There is, to be sure, a large and growing empirical literature on the Cuban missile crisis. Some of this literature is quite new, drawing on evidence only recently made available to the public (see Allyn, Blight, and Welch 1989/90; Blight, Nye, and Welch 1987; Bouchard 1991, ch. 4; Brugioni 1991; Garthoff 1988, 1989; Hampson 1984/85; Lukas 1987; Trachtenberg 1985; and Welch and Blight 1987/88). But for the most part, we will not address empirical issues raised by Allison's analysis.

A TYPOLOGY OF MODELS

Before plunging into a detailed analysis of Allison's three models, it is advisable to distinguish one from another by defining their key properties. As several early critics of Allison's work noted, however, this is more difficult than one might think (see, e.g., Cornford 1974, 241; Wagner 1974). It seemed particularly difficult to disentangle Models II and III, Wagner arguing that "it is not entirely clear whether Model III is independent of Model II or an extension of it; certainly bureaucratic bargaining seems to be constrained by many of the factors discussed under Model II, and many of Allison's readers seem to mingle the two together in speaking of the lessons of his book" (1974, 448). Hence, we will begin by asking a very general and deceptively simple question about Allison's three models: On what assumptions is each one based?

Four different kinds of assumptions appear to be important, and they lead to a typology of models of policymaking useful for characterizing Allison's three models. The first class of assumptions concerns the number of actors. The classical approach to the study of international relations postulates that the government of a sovereign state behaves as if it were a unitary actor. Allison's contrasting approach, exemplified by his Models II and III, is to decompose the government into multiple actors. Note, however, that a shift to multiple actors immediately requires specification of whether the multiple actors have the same or conflicting goals. Very different models—and very different understandings of policymaking—follow from different assumptions here. Though Allison is less explicit here than he might be, he appears to assume in both Models II and II that the actors' goals are in conflict. But even if actors have the same objectives, this does not mean that policymaking is a trivial problem. As any student of bureaucracy knows, even when there is no serious conflict over goals, coordinating the actions of a large number of executive branch actors is no easy task. Thus, a model could logically—and quite meaningfully—stipulate multiple actors whose objectives are the same. Hence, when we consider the number of actors posited by a model, a second type of assumption, namely, whether or not the actors' goals are the same, is necessarily involved. If a central goal of *Essence of Decision* is to understand the relative virtues of government-as-single-actor versus government-as-multiple-actors models, the results of the comparison will be misleading if this second kind of variable is neglected.

The third class of assumptions concerns the degree of rationality attributed to decision makers. As Allison stresses, the classical approach assumed that decision makers are rational. In contrast, students of behavioral organization theory like Herbert Simon

Figure 1

A Typology of Policymaking

DECISIONMAKERS
ARE

	Perfectly rational		Imperfectly rational	
Single Decisionmaker	1a complete information	1b incomplete information	2a complete information	2b incomplete information
Many Decisionmakers, same goals	3a complete information	3b incomplete information	4a complete information	4b incomplete information
Many Decisionmakers, conflicting goals	5a complete information	5b incomplete information	6a complete information	6b incomplete information

and James March, as well as political psychologists like Robert Jervis and John Steinbruner, maintain that all agents have limits to their cognitive capacities. Though there is a continuum of possibilities here, we can simplify by dichotomizing. A model could stipulate either that agents are perfectly or imperfectly rational.

Finally, a model may assume that decision makers are perfectly rational yet also postulate that they are imperfectly informed, either about which outcomes will occur (prospective uncertainty) or about what has already transpired (retrospective uncertainty).[3] It seems intuitively obvious—and here explicit theorizing supports intuition—that either informational problem can strongly affect the predictions of models. Thus, the fourth kind of assumption concerns the amount of information attributed to decision makers. We simplify by assuming that with "complete information" there is neither retrospective nor prospective uncertainty. Thus, all players know the structure of the game (including payoffs), who has done what in the past, and the outcome that will result from any set of moves. With "incomplete information" there can be either retrospective or prospective uncertainty, or both.

These four classes of assumptions can be combined to yield the typology of models in Figure 1. The typology will help us identify various possible models of the policy process. To serve this purpose, a typology's categories should be mutually exclusive (overlapping categories make identification uncertain) and collectively exhaustive, so that any candidate can be "typed" as an instance of some class. A quick inspection of this typology reveals that the cells are indeed mutually exclusive and collectively exhaustive.

With this typology we can now attempt to characterize Allison's three models. Model I, the rational actor model, clearly belongs in cell 1. Indeed, the overwhelming bulk of Allison's discussion in *Essence of Decision* suggests placement in cell 1a, with its assumption of a single, perfectly rational actor with complete information. We will soon argue, however, that the complete information assumption neglects what international relations theorists have identified as a central aspect of their field, namely, actors' pervasive uncertainty about the capabilities, motives, and actions of others. Considerable insight, in other words, is to be gained by extending Model I into cell 1b, the realm of incomplete information. Further insight into the workings of single-actor models could be obtained by relaxing the assumption of perfect rationality, thus moving into cell 2. As examples we might cite psychological analyses of individual bounded rationality in foreign policy decision making (e.g., George 1980, chaps. 2–3; Jervis 1976).

At first glance, it might seem that Model II, the organizational process model, belongs in cell 4b. After all, boundedly rational actors, facing uncertain environments and using simple adaptive strategies, are the heart of this model. However, at several places in his discussion of Model II Allison mentions conflicting goals. For example, he refers to "parochial priorities," the "quasi-resolution of conflict," and organizational "imperialism" (1971, 81–82, 93). He even refers to agenda manipulation: "The short list of alternatives [generated by an organization] reflects not only the cost of alternative generation but, more important, each organization's interest in controlling, rather than presenting, choices—for example, by serving up one real alternative framed by two extremes" (p. 90). One can also find, in his discussion of Cyert and March (1963), references to organizations as a "coalition of participants" and "bargaining among potential coalition members" (p. 76). Thus, Model II might appear more properly to belong somewhere else, perhaps in cell 6.

As an empirical observation, an assumption of conflicting goals in an organizational process model is unassailable. For most important issues (virtually by definition of *important*), organizations and their leaders disagree about objectives. However, we do think that some interesting hypotheses—particularly about the kinds of problems involving bureaucratic coordination that Allison discusses in regard to Model II—could be generated by "withdrawing" into cell 3. This class of models is ideal for examining pure coordination problems among perfectly rational actors. Anyone who thinks that pure coordination problems are trivial should consider Schelling's examples, which only scratch the surface of coordination problems (1960, 54–58). If different members of a team observe different aspects of the environment and these environmental signals are noisy, formidable communication and division-of-labor problems typically arise.[4] Even in the context of goal consensus and perfect rationality (i.e., each agent, like a perfect Bayesian statistician, extracts the maximal amount of information from each noisy observation), there remain structural issues of who should communicate what to whom.[5] If we make the problem dynamic (e.g., involving optimal adaptation to a changing environment), the complexities multiply—even for actors with the same goals. Understanding of these particular issues is probably hindered, not helped, by construction of models involving conflicting goals.

The heart of Model III is multiple decision makers with conflicting goals—hence the third line of the typology. However, as Harrison Wagner pointed out, it is unclear whether this model assumes perfect or bounded rationality: "Model III . . . contains no discussion of behavioral assumptions at all. Yet one could presumably construct a theory of bureaucratic bargaining based on the decision theorists' assumptions, and another based on Herbert Simon's" (1974, 448).

Allison's own summary of the core of Model III (1971, 162) emphasizes bargaining and other obviously political processes as central to policymaking and deemphasizes issues involving information processing. Moreover, his discussion of power involves actors who are apparently able to make complex strategic calculations about how to maximize their influence (pp. 168–69). These actors thus seem to have the cognitive capacity usually assumed by rational choice models of policymaking, so that readers might conclude that classical rationality is presumed for Model III, leading to its placement in cell 5.[6] Moreover, since Allison explicitly states that bureaucratic politics games "are not played under conditions of perfect information" (p. 178), Model III seems to belong in cell 5b.

However, other of Allison's statements about Model III suggest a different classification. For example, Allison refers to foul-ups ("e.g., choices that are not made because they are not recognized or are raised too late, misunderstandings, etc." [pp. 145–46]); the limited attentional capacities of decision makers (pp. 176–78); and problems of misperception, misexpectation, and miscommunication (pp. 178–79). Of incrementalism in decision making, he writes "Analysis is drastically limited. Important policy outcomes are neglected. . . . By proceeding incrementally and comparing the results of each new policy with the old, actors reduce or eliminate reliance on theory" (p. 154). All these characteristics bear a strong family resemblance to the decision processes of ordinary, imperfectly rational humans.

In terms of our typology, then, Model II (which initially might seem to belong in cell 4, probably 4b) can also be classified in cell 6, perhaps even 6b. Model III, which might seem to belong somewhere in cell 5 (probably 5b) can also be classified in cell 6, perhaps even 6b. Thus, we find that Models II and III, which have different intellectual pedigrees and which Allison presumably intended to be distinct, apparently share much of the same analytical turf.

We can see at the outset, then, that our simple typology raises some serious questions about just what is driving each of Allison's models. We do believe that Allison's initial instincts were on the right track. As our typology suggests, there is room for several different classes of models, and it is an essential enterprise to assess the relative explanatory and interpretative power of models in each of these different classes. It is necessary, however, to specify much more precisely what is assumed for each of these models. For this reason, Allison's subsequent enterprise of *combining* Models II and III into one overarching *bureaucratic politics* model (see Allison and Halperin 1972) is probably a step in the wrong direction. Combining two theories before clarifying their defining properties can easily produce inconsistent claims (Landau 1972, 227). Hence, we are inclined to follow Cornford's (1974) lead in arguing that there are virtues in separating Models II and III and in clarifying their internal logic prior to synthesizing.

We close by noting that identifying a particular model as belonging, say, to cell 1a does not necessarily yield a detailed prediction about what policy outcomes to expect from the model.[7] Such an identification does have certain implications—for example, that the relevant government will not encounter surprises (by virtue of the complete information assumption) and will exhibit transitive preferences (the unitary actor and rationality assumptions). But such predictions fall well short of the kind of policy-relevant claims naturally sought by students of foreign policy. The burden of generating falsifiable predictions lies on the models, not on the typology.

ANALYSIS OF MODEL I

Allison's analysis of rational decision making is the most coherent and most easily understood of his models. A rational actor model in international relations should have two essential components: a decision-theoretic one and a game-theoretic one. The former covers attributes of the decision maker(s), the latter, strategic interactions. We have two major points to make about Allison's rational actor model. First, the decision-theoretic component is too simple: one can easily construct a far more complex rational actor that better reflects the complexities of real decision makers and the choice situations they confront. Second, the model neglects strategic interactions.

Before examining the details of Model I, we must discuss one difficulty with analyzing Allison's treatment of the model: his tendency to distance himself from his own presentation. As he notes "Since the purpose of the Model I account is to present a strong, typical explanation, it seems fair to let the account reflect only conventional evidence plus additional facts that the Model itself would naturally uncover" (1971, 248). This raises the problem whether one should critique (1) the classical version of single-actor rational choice that scholars have traditionally used, (2) Allison's interpretation of this standard model, or (3) what Allison himself thinks this model should look like if (given its premises) one developed and applied it "properly." We are inclined to think that approach 1 or 2 best characterizes Allison's treatment; Allison never tells the reader what he thinks Model I would be like if properly developed. This matters because we think a more fully developed version of Model I would have substantially more explanatory power than Allison's Model I. For simplicity, we will treat the model as Allison's own and criticize it as such, understanding that it has a different status in Allison's thought than his Models II and III.[8]

Decision-theoretic Aspects

Model I employs two key decision-theoretic assumptions about nation–states, postulates found in most canonical formulations in international relations. First, the nation can be treated as a single actor endowed with a set of goals and the capacity for pursuing these goals. Second, this single actor pursues these goals rationally. Though it is easy to criticize the assumption that the nation is a single actor, many abstract analyses require treating an aggregation of human beings as a single unit. For some purposes such aggregation is useful and appropriate. We will, instead, focus our attention on Allison's treatment of rationality.

In common parlance and in Allison's usage, rational decision making consists of four key steps: define one's goals, list all options for achieving these goals, evaluate each option in terms of the extent to which it achieves the goals, and choose the option that best achieves the goals. As Allison points out, students of international politics and foreign policy have long used this model (if only implicitly). He demonstrates how the model might be used to make sense of three major decisions in the missile crisis: why the Soviet Union placed the missiles in Cuba in the first place, why the United States responded with a naval quarantine, and why the Soviet Union withdrew its missiles.

Allison's rational actor model is as simple a version of rational choice as one could imagine. His purpose in formulating this model was to show how something much like it underlies many different bodies of thought in international relations. But in this particular enterprise lies what we see as the model's major fault. It is *too* simple, for it omits several significant properties of rational actors and important problems they face. The model can be enriched in several different ways.

Goals. An ambiguous aspect of Model I is whether rational actors are presumed to have just one goal. Before explicating the model, Allison notes that the Model I theorist "presents an argument for *one objective* that permits interpretation of the details of Soviet behavior as a value-maximizing choice" (1971, 11; emphasis added). Later he discusses how this analyst "produced an argument for *one goal* (rectifying the nuclear balance) that made the Soviet emplacement plausible" and "proceeded as if his assignment had been: make a powerful argument *for one objective* that permits the reader to see how, given the strategic problem, if he had been playing the Soviet hand, he would have chosen that action" (pp. 246, 253; emphasis added). And in applying Model I to the Soviets' decision to install the missiles, Allison's discussion appears largely oriented toward inferring which *one* of the five possible goals best explained the Soviets' decision. For example, in ending his discussion of Cuban defense as a possible motive, he concludes, "Cuban defense might have been a subsidiary effect of the Soviet gamble, but not its overriding objective" (p. 50).

Yet in discussing the international relations theorists Hans Morgenthau and Raymond Aron, Allison notes in particular, "Criticizing the attempts of theorists such as Morgenthau to explain national action by reference to a single goal, Aron argues that governments pursue a spectrum of goals, tempered by 'the risk of war [that] obliges [them] to calculate forces or means' " (1971, 27). And in his presentation of Model I, Allison seemingly allows the possibility that a rational actor might have multiple goals: "National security and national interests are the principal categories in which strategic goals are conceived. Nations seek security and a range of other objectives" (p. 33).

Although Allison does not explicitly discuss this matter of multiple goals, it raises important issues in understanding and applying Model I. Nothing whatsoever in the theory of rational action requires an actor to have just one goal. This is a significant feature of the theory, for empirically, an actor with only one goal is odd indeed. In fact, we normally use the term *monomaniacal* to refer to the *irrationality* of people motivated by only one objective. Typically, we expect rational actors to have multiple goals. Indeed, nothing in the axioms of rational action states that it is nonsensical to harbor multiple goals. The goals may conflict. But in a rational choice model a utility function is postulated to resolve all *intra*personal conflict.[9] Thus, rationality lies in the considered and consistent pursuit of whatever goals one happens to have.

We can support our arguments by reference to the Soviet Union's possible motives for placing nuclear missiles in Cuba. Presumably there was, as Allison concluded, a strong concern for the strategic balance. The recent U.S.–Soviet conferences tend to bear this out (Blight and Welch 1989). However, it is not unreasonable to think that the Soviets were also concerned about defending Cuba. The Bay of Pigs invasion had given the Soviets (and the Cubans) cause for concern. Moreover, the Kremlin would have been pleased if placing missiles in Cuba would have diminished the prestige of the United States in the eyes of West Europeans and nonaligned countries.

The Soviets' concern for the strategic balance may well have been of overriding importance; other concerns may have been of negligible significance in their minds. Nonetheless, it is reasonable to think that the Soviets would have been less inclined to place the missiles in Cuba if this action would have clearly *impaired* these other goals. Thus, these other goals cannot be considered irrelevant to an understanding of the Soviets' motives and choices.

For each of these other goals, Allison may be correct in arguing that there was some other action better than placing nuclear missiles in Cuba. However, the bare fact that the chosen action is not *optimal* for the other goals (such as defending Cuba) does not imply that these other goals were completely irrelevant to the decision.

Time. Allison's rational actor model is characterized by a single-time-period orientation. Thus, the rational actor considers a problem occurring at a point in time; and once an alternative is chosen and implemented, the matter is over. Allison himself mentions this feature as a limitation of the model, labeling it "static selection."

One might argue that a single-time-period model is appropriate for crises. However, even in a crisis, the effects of options may be spread over multiple time periods. Consider, for example, President Kennedy's concern for the United States' international reputation for firmness and resolve: such reputational consequences may endure for years. Thus, in estimating the effects of any option, a rational agent must aggregate, into a single measure, costs and benefits that may be spread over a long time. Aggregating streams of costs and benefits is a rather different kind of choice problem than aggregating costs and benefits that all occur at the same time.

Incorporating multiple time periods in a model of rational choice leads directly to the issue of time preferences. Is the rational actor patient, valuing distant benefits nearly as much as current ones, or impatient, sharply discounting benefits that are enjoyed long after the moment of choice? Note that actors with identical goals but different time preferences would value the same option differently, due to their different degrees of impatience.

Uncertainty. Allison's rational actor model also ignores the problem of uncertainty. While Allison obliquely considers uncertainty in his discussion of Model II, the problem is completely neglected in Model I. This is a striking omission, since the traditional literature in international relations, which Allison cites as having implicitly used rational actor models for centuries, emphasizes how a state's uncertainty about other states' goals and capabilities shapes its own choices.

There do exist formal models of rational choice that show how an actor should make optimal choices under uncertainty. Some of these models were formulated well before Allison conducted his study. (Indeed, he cites relevant texts on statistical decision theory and game theory [1971, 285, n. 87].) Thus, several different types of uncertainty were analyzed and well understood by rational choice theorists before Allison's article or book was written. One kind of uncertainty that was well understood was a nonstrategic, prospective type, involving exogenous and unpredictable acts of nature. Though probably less important than the strategic aspects considered below, they were not irrelevant in the Cuban missile crisis. Bad weather, for example, would have made a surgical air strike more difficult. Unexpected equipment problems, such as those that plagued U.S. helicopters in the aborted Iranian hostage operation, also fall under the domain of statistical decision theory. No rational military planner would assume that all his equipment will work with certainty. And these problems can be sensibly addressed within the confines of the single-actor assumption. Indeed, statistical decision theory classically developed under this assumption, as did the related probabilistic methods in operations research.[10]

Observe that just as introducing multiple time periods raises the issue of decision makers' time preference, introducing uncertainty raises the issue of decision makers' attitudes toward risk. Whether they are risk-averse, risk-neutral, or risk-seeking must be specified in order to complete a rational actor analysis. However, it must be recognized that these issues, though analyzed in more recent international relations theories (e.g., Bueno de Mesquita 1981, 1985), were not salient in rational choice theorizing in political science prior to *Essence of Decision*.

In addition to uncertainty about states of nature, it is empirically clear that U.S. decision makers were very uncertain about the Soviets' motives for placing missiles in Cuba. Allison himself pointed up this problem, describing the markedly different U.S. interpretations of Soviet behavior (1971, 40–56).[11] These interpretations placed the Cuban adventure in the context of larger Soviet strategies, and the magnitudes of these strategies' payoffs were hotly debated by U.S. policymakers.

However, game theory was poorly equipped to address this kind of uncertainty when Allison wrote *Essence of Decision*, and one's evaluation of the book must take this history into account. But over the last dozen years, much of the frontier work in game theory *has* addressed the issues of incomplete information about another player's preferences and imperfect information about a player's past actions. Moreover, this kind of analysis has diffused into formal models of foreign policy crises (Morrow 1989; Powell 1990; Wagner 1989). In this respect, therefore, Allison's rational actor model is seriously out of date.

Game-theoretic Aspects

In international relations, the prime motivation for the unitary rational actor assumption is to conserve the intellectual resources of scholars and to focus these resources on analyzing the strategic relations among nations. It is therefore a surprising and serious omission that Model I does not explicitly examine how other nations might react to a state's moves.

To be sure, Allison's description of the U.S. decision to institute a naval quarantine is replete with references to possible reactions by the Soviets. This, however, is in chapter 2, where he *applies* Model I to the crisis. In chapter 1, where the model itself is presented, there is relatively little in his informal analysis—and nothing in his rational actor model—describing how a rational actor should think through the possible countermoves that an adversary might take in response to the first actor's moves.

Indeed, in chapter 1 Allison persistently blurs the distinction between decision theory and game theory. Under the head "A Rigorous Model of Action," presenting the analytical core of Model I, he writes: "In modern statistical decision theory and game theory, the rational decision problem is reduced to a simple matter of selecting among a set of given alternatives, each of which has a given set of consequences: the agent selects the alternative whose consequences are preferred in terms of the agent's utility function which ranks each set of consequences in order of preference" (1971, 29). And in several other places he refers to the basic "value-maximizing" postulate of Model I (pp. 30–31, 34, 36).

It is true, of course, that a basic premise of decision theory is that actors maximize utility or (in risky circumstances) expected utility. The idea of choosing the alternative that the decision maker prefers above all others makes sense when that decision maker is isolated from all other people. These are the so-called Robinson Crusoe problems (Tsebelis 1989). But clearly, most of the important choices in the Cuban missile crisis were made in a strategic setting—outcomes depended on the behavior of both governments, thus intertwining the fates of the two nations. The relevant branch of rational choice theory for such situations is game theory, not decision theory. And in the branch of game theory that is most relevant to the anarchic context of international politics (noncooperative game theory, which assumes that players cannot make binding agreements), actors are not presumed to maximize utility in any simple, decision-theoretic sense.

Figure 2

The Prisoner's Dilemma

		Player 2	
		Cooperate	Defect
Player 1	Cooperate	R, R	S, T
	Defect	T, S	P, P

Prisoner's Dilemma Assumption: T>R>P>S

The fundamental prediction of noncooperative game theory is that the outcome of a strategic interaction will be a Nash equilibrium, defined as follows: Given the choice of player 2, player 1 is doing as well as possible; and given the choice of player 1, player 2 is doing as well as possible. Yet the idea of a Nash equilibrium is never mentioned in *Essence of Decision*. The analysis is purely in terms of the value-maximizing choice of an isolated decision maker (1971, 29–31, 34, 36), and there are several important reasons why the outcome of a Nash equilibrium may not be transparently linked to value maximization. We consider three of these.

Games with Undesirable Equilibria. First, *all* the players in a game may dislike the outcome of a particular Nash equilibrium. This can happen not only in a subjective sense (both parties prefer the whole loaf to the half each agreed to take) but in a stronger, objective sense. A Nash equilibrium can be Pareto inferior, that is, there may exist another (feasible) outcome that *both* players prefer to the Nash equilibrium.

This point is particularly important in games where the Nash equilibria are deficient by the Pareto criterion. The outstanding example of this type of game is, of course, the prisoner's dilemma (Figure 2). If this game is played only once, the only Nash equilibrium is the Pareto inferior outcome of (P, P). In such a situation there is no transparent connection between each player's preference ordering ($T > R > P > S$) and the outcome. Not only is the outcome of mutual defection ranked third by each player, but *both* players prefer the payoff deriving from mutual cooperation.

Games with Multiple Equilibria. Second, in many situations it is impossible for both actors to get their most preferred outcomes, even if the Nash equilibria are Pareto optimal. A well-known example of this in international relations is the game of chicken (Figure

Figure 3

A Game of Chicken

		Player 2	
		concede	aggress
Player 1	concede	0, 0	-3, 8
	aggress	8, -3	-10, -10

Figure 4

A Game with a Mixed Strategy Equilibrium

		USSR	
		Put missiles in Cuba	Do Not Put Missiles in Cuba
U.S.	Intensive Inspection	1, -1	-1, 1
	Casual Inspection	-1, 2	2, 0

3). There are two Nash equilibria in chicken: (*aggress, concede*) and (*concede, aggress*). In the first outcome, player 1 gets its most preferred outcome, and player 2 is stuck with its third best. In the second equilibrium, matters are reversed. The presence of these two equilibria, with their markedly different payoffs, points up another important difference between decision- and game-theoretic analyses. If there is only a single decision maker, then there is either a unique alternative that is value-maximizing or several alternatives. Neither case presents a problem; for even if there are several such alternatives, they must by definition yield the same payoff and are therefore equivalent. But in a multiperson situation, the equilibria need not be equivalent; they can differ sharply in both relative and absolute senses. Accordingly, the players in chicken are not indifferent between the two equilibria.

Hence, once again, there is no simple relation between the game-theoretic notion of equilibrium outcomes and the decision-theoretic idea of value maximization. It is true that *if* player 1 aggresses, player 2's value-maximizing choice is to concede. But player 2 would itself be better off aggressing—so long as player 1 concedes. Thus, contrary to Allison's claim that "in modern statistical decision theory and game theory, the rational decision problem is reduced to a *simple matter* of selecting among a set of given alternatives" (1971, 29; emphasis added), chicken exhibits an intricate strategic structure. Indeed, due to its complexity—the potential for mutual disaster if each side tries to get its maximal payoff or mutual accomodation if both sides try to avert disaster—predicting the outcome of chicken is by no means trivial. *Any* of the four outcomes could plausibly occur. Whether this is a defect of game theory in not making a falsifiable claim for this game or an indeterminacy inherent in the situation, we cannot say. What we can say is that just assuming rationality does not, in itself, reduce the decision problem to a "simple matter."

In our experience teaching models of politics, most students begin a course believing that preferences and outcomes are tightly linked in rational choice models and that the paradigmatic rational choice answer to the question, "Why did this outcome occur?" has the form, "Because actor so-and-so wanted it."[12] Such explanations do not come close to tapping the potential of rational choice analyses. They completely overlook the complexities arising both from strategic interaction and from the institutional context of the game. Indeed, by slighting such complexities, these uses of rational choice theories *degrade* their value; much of the intellectual benefit of rational choice analyses is that by black-boxing inner mental processes, we can concentrate our attention on relations among decision makers.

Endogenous Uncertainty in Games of Complete Information. The preceding remarks about the game of chicken informally suggest that game theory might view some interactions as being fundamentally indeterminate. We will now sharpen this point. It has been well known for decades that uncertainty can arise *endogenously* in strategic situations. To illustrate the point squarely in the matter at hand, consider the representation of one aspect of the Cuban missile crisis (Figure 4).

In the situation shown in Figure 4, if the United States carries out intense inspections, the USSR would prefer not to put the missiles in Cuba ($1 > -1$). However, if the USSR holds back, then the United States would rather not incur the cost of intense inspections ($2 > -1$). But if the United States conducts only cursory inspections, then the USSR does want to install the missiles ($1 > 0$). This, however, takes us full circle, since if the USSR is going to install the missiles, the United States wants to inspect closely ($1 > -1$). Hence, in this kind of situation, there is no equilibrium in pure strategies; but there is an equilibrium if each player uses mixed strategies, that is, plays probabilistically.[13] Thus, uncertainty will arise endogenously from the strategic logic of the situation. The only equilibrium in this circumstance is where each side keeps the other guessing about its course of action, just as in the child's game of stone–scissors–paper.[14]

It is evident, therefore, that strategic uncertainty was relevant in the crisis and that some theoretical tools were available to aid analysis. (For a recent analysis of the Cuban missile crisis using a model of strategic uncertainty, see Wagner 1989.)

Many readers of *Essence of Decision*, we suspect, conclude that models of states as unitary rational actors provide an inadequate account of this crisis. Such a conclusion is not well founded. Allison's version of a rational action explanation is so oversimplified that it should not have been *expected* to perform well.

ANALYSIS OF MODEL II

Allison's discussion of Model II is one of his strongest sections. He extracts several insights from organization theory and applies them to the missile crisis in interesting ways. For example, though the Soviets may have had sound engineering reasons for building the missile sites in their characteristic pattern, Model II suggests an interesting alternative explanation—it was simply a bureaucratic routine being enacted. In the study of foreign policy making and implementation, Allison's chapters on bureaucratic routines were pioneering efforts.

Nonetheless, we have some reservations about his analysis. The core of Model II is the idea, developed by Herbert Simon and James March, that individual decision makers are boundedly rational (see Allison 1971, 71). We have two main concerns with Allison's interpretation of this theoretical core. First, Model II assumes that imperfectly rational agents would usually use simple decision rules (p. 83). In itself, this assumption is consistent with much of the writing of the Carnegie School.[15] But Model II goes beyond this premise by suggesting that these simple rules, or *standard operating procedures*, sharply limit and constrain *behavior*, that is, that simple rules generate simple, predictable behavior (pp. 78–79, 83, 87–91). We believe that this conclusion greatly underestimates how complex behavior—of even a single decision maker—can arise out of the use of simple rules. Second, suppose we grant that *individual* decision makers are sharply constrained in their cognitive abilities. What does this premise imply about how constrained *organizations* are? Consistent with the viewpoint of Simon's *Administrative Behavior*, we argue that organizations can significantly ease the constraints on information processing that confront individuals (1947, 79–80). Taking these two points together, our position is that the use of simple decision rules by individual decision makers does not imply that the behavior of an organization will be simple, unsophisticated, or predictable. Thus, we agree with March and Simon, who argue that "complex processes can be aggregated from simple elements" (1958, 178).

Decision Rules Versus Behavior

Allison's basic image of organizations in Model II is that of simple, predictable behavior generated by simple rules:

> Major lines of organizational action are straight—i.e., behavior at one time, t, is marginally different from behavior at $t - 1$. Simple-minded predictions work best: behavior at $t + 1$ will be marginally different from behavior at the present time.
>
> The preeminent feature of organizational activity is its programmed character: the extent to which behavior in any particular case is an enactment of preestablished routines.
>
> The fact that the fixed programs (equipment, men, and routines that exist at the particular time) exhaust the range of buttons that leaders can push is not always perceived by these leaders. (1971, pp. 91, 81, and 79, respectively)

This image resonates strongly with our intuitive conceptions. We are all familiar with the stereotype of an inertial bureaucracy mindlessly following its procedures regardless of consequences. As with many popular images, this one is not completely wrong—far from it. The picture of simple, rule-governed action provides important insights into organizational behavior. However, precisely because the image is so intuitive, we must be careful. Familiarity in model building breeds laziness, if not contempt. The notion that simple rules generate simple behavior is so commonsensical that it is easy not to question it. Yet the link between rules and behavior is more subtle than is captured by either Model II or its associated popular image. Complex behavior *can* emerge from simple rules. We now describe six ways this can happen.

Varying, and Possibly Stochastic, Inputs. Many choice rules work on inputs—information, people, objects—that vary significantly. This variability, in tandem with even a simple rule, can produce complex behavior. A good example of this is the behavior of one of the simplest strategies in the prisoner's dilemma, Tit for Tat (TFT). This strategy—cooperate on the first round and thereafter play what one's partner did on the previous round—is only a tad more complicated than the *simplest possible* rules (always cooperate or always defect (all-D)). However, whereas those unconditional strategies generate behavior as simple as themselves (regardless of their strategic environment), in variegated environments TFT will behave in correspondingly complex ways. Against all-D, TFT cooperates once and never again; against a nice partner TFT cooperates forever; against a sneaky nasty strategy, TFT will cooperate as long as its partner does, retaliating against a defection but then returning to cooperation if its sneaky partner apologizes. Moreover, if the environment is noisy (unobserved random disturbances affect the players' payoffs), TFT produces variable behavior even when playing an equally simple strategy—itself (Bendor 1987).

Because simple rules can be activated by random shocks in an organization's environment, its behavior can be, in a certain sense, surprising, as well as complex. For example, disaster relief could be highly routinized and preprogrammed, but the organization's behavior would not be *unconditionally* predictable. For these organizations, it need not be true that the best predictor of the organization's behavior at time t is its behavior at $t - 1$.[16]

Recursive Rules. Many public agencies seem to follow a simple history-dependent rule of the form, decision $t + 1 = f(\text{decision } t)$. Such rules are called *recursive*. To figure out what the decision will be in period $t + k$, in terms of the decision made in period t, one just reapplies the rule $f(\cdot)$ over and over again. Thus, decision $t + 2 = f(\text{decision } t + 1) = f[f(\text{decision } t)]$, and so on. Do such recursive choice rules always generate simple behavior? They do not. Interesting developments over the last 10 years in a field of mathematics known as *chaos theory* have shown that if a recursive rule is nonlinear, it can create a pattern that is so complex as to appear random, even though the rule itself is completely deterministic. For example, consider the simple recursive rule $x_{i+1} = k \cdot x_i(1 - x_i)$, where k is a constant between 0 and 4. Let the initial value x_1 be between 0 and 1. How will x behave over time? It turns out that if k is close to 4, the behavior of x is "chaotic," wandering all over the [0, 1] interval and never settling down. For example, let $k = 3.7$ and $x_1 = .4$. Then the next 10 values of x_i (rounded to four places) are .888, .368, .8605, .4441, .9134, .2926, .7658, .6636, .8259, and .5319.

Merely inspecting these values of x_i, one would probably attribute them to a stochastic process. Yet the underlying equation is not only deterministic but also very simple, being only one degree higher than linear. Moreover, it is not a historically rich rule, for it depends only on the decision in the previous period. Real precedent-governed agencies have a much more complex memory structure.

Simple Linear (Though Probabilistic) Rules. Since Model II repeatedly refers to the linear nature of organizational behavior, one may object to the previous example because it is nonlinear. We respond in two ways. First, linearity is an empirical hypothesis; we have no a priori guarantee that organizations typically behave linearly. Second, we now demonstrate, via another example, that behavior can be surprisingly complex even if linearity is retained.

Consider the following model of experiential learning. Suppose an administrator has two options, x and y, and knows that neither one is sure to work or doomed to fail. Objectively, the probability that x succeeds is p, the chance that y works is q, where $0 < q < p < 1$. (They need not sum to one.) These probabilities are constant and independent over time. In the first period, the decision maker has a propensity to try x denoted $p_1(x)$ and a propensity to try y denoted $p_1(y) = 1 - p_1(x)$. Over time, the administrator adapts via a simple learning rule. If in period i x was tried successfully, the administrator's propensity to try x again is strengthened:

$$p_{i+1}(x) = p_i(x) + a[1 - p_i(x)].$$

The parameter a represents the rate of learning and is between 0 and 1. If x failed, the administrator's propensity to try it weakens: $p_{i+1}(x) = bp_i(x)$, with b also between 0 and 1. The equations for alternative y have the same form. Note that these equations are linear.

However, whereas an expected utility maximizer will behave simply in the long run—choosing x always—a decision maker using this linear adaptive rule will never settle down on either alternative. Instead, the adaptive decision maker will use alternative x in the long run $(1 - q)/[(1 - p) + (1 - q)]$ proportion of the time and y the remainder. Thus, the simple linear adaptive rule produces more complex behavior than does the optimal strategy.

Combinatorial Effects. Several times in *Essence of Decision,* Allison uses the metaphor of chess when discussing how leaders are constrained in their choice of options. This is odd, because chess is the paradigmatic example of a choice situation that involves only a handful of basic rules yet exhibits truly Byzantine strategic complexity. There are only six distinct pieces in chess; each moves in only a few ways. (We may consider each piece, with its legal moves, as equivalent to a standard operating procedure.) Therefore, each player is "limited" to shuffling and reshuffling this combination of standard operating procedures over the 64 possible locations.

But the quantitative impression thereby conveyed, namely, that the limited number of procedures creates a roughly equal (even in respect to order-of-magnitude) number of moves is grotesquely inaccurate. As is well known and as Allison himself noted, the number of possible sequences of play (i.e., behavior) in chess is staggeringly large (1971, 286, n. 93). A common estimate is 10^{120}, a number so large as to be equivalent, for all practical purposes, to infinity. Human beings can never exhaust the richness of chess. This is pure combinatorial explosion: the rules are deterministic. Nor does the complexity depend upon stochastic inputs. It is simply a matter of stringing together the handful of rules to generate new positions, whence one reapplies the basic rules, and so on. The example of chess suggests that our intuitions grossly underestimate combinatorial complexity.

Moreover, when we compare chess to the strategic maneuverings of two real military forces (to return to *Essence of Decision*'s empirical domain), the odds are good that chess is *simpler*. No matter how one counts distinct "pieces" in, say, a navy, there are surely more than seven! No matter how crudely one wants to count the basic standard operating procedures of, say, a destroyer, it has more than the handful available to even the most complex chess piece. And the "board" of any moderately large battle is more variegated than the (nontopographical) eight-by-eight chessboard. Therefore, chess, the paradigmatic choice environment of behavioral decision theory, provides a *lower bound* for the complexity of behavior one would expect to see in a clash between two governments. Thus, in this instance *Essence of Deci-*

sion seriously misapplied the lessons of the Carnegie School.

Hierarchies of Procedures. In a sense the linking of procedures into a larger unit (creating a "program," in Allison's terminology) is just a type of combinatorial complexity. In this sense the remarks just made apply. However, hierarchies of rules create another way for complex behavior to arise from simple rules. Suppose the Department of Defense used the simple budgeting rule, "Every year we allocate 10% of our budget to research and development." Naturally, this rule would be accompanied by a host of procedures concerning for example, how the designs of weapon systems should be specified. For our purpose, we focus on the relation between this one rule and consequent behavior.[17]

Again, the rule's simplicity is deceptive. The allocation of a portion of the Defense Department's resources to research and development has created a historically unprecedented number of new weapons, introduced at an equally unprecedented rate. Each new weapon is accompanied by its own standard operating procedures, as well as the procedures "frozen into" the hardware itself. Thus simple high-order rules can generate new lower-order ones. (For a discussion of this point, see March and Simon 1958, 150, 170.)

Large Sets of Rules. Our first three points showed how a single rule could produce complex behavior. The next two points showed how a few simple elemental rules could aggregate into complex behavior. Here we wish to underscore an obvious empirical point: even if we set aside combinatorial complexity, some agencies have a great many rules. Consider the Internal Revenue Service code. The gradual, barnaclelike accumulation of exemption piled upon exemption, modification upon modification, has created a code of rules that is awesomely complex.

Very large sets of rules create problems for scholars, as well as citizens. An observer may find it difficult to predict organizational behavior because of not knowing *which* rule will be evoked out of the set of all possible rules. There are two related reasons for this. First, large sets of rules increase the possibility that two or more rules may conflict: more than one rule will apply to any one situation, and it will be unclear which has priority. Second, though many organizational rules have an *if/then* structure like computer programs ("If condition x obtains, apply procedure y"), the conditional part of the rule—the description of condition x—is often much less precise than in computer programs. This also makes it more difficult to predict which rule will be evoked.

The classic illustration of both problems—conflicting rules and imprecise conditionals—is the legal system. If one regards the judiciary as an organizational system, the model of rule-governed behavior seems apt. A judge's behavior is, after all, supposed to be informed by legal rules. But even knowing the whole set of rules that could guide the judge's decision may not enable one to predict behavior, because one does not know *which* of the many legal precedents the judge will select. Again, behavior is unpredictable despite the stable set of rules. The test is not, Did the decision follow from an organizational rule? The test is, Could one have *predicted* which organizational rule would be evoked?

It should be understood that we are not here advancing empirical hypotheses. We are not asserting that any particular set of officials or agencies use any of the rules described in our first five points. Our argument is theoretical. The relation, implicitly assumed by Model II, between simple rules and simple behavior is weaker than one might think. The examples illustrate that rules can generate relatively complex behavior even when the rule is deterministic (points 1 and 2) or linear (point 3) or the game is isolated from stochastic shocks (point 4). Accordingly, the suspicion grows that simple rules will produce simple organizational behavior only if (1) the rules are deterministic *and* (2) the rules are linear *and* (3) the game is isolated from stochastic shocks *and* (4) individual choice rules are not aggregated in a combinatorially rich way *and* (5) individual choice rules are not grouped hierarchically so that higher-level rules generate new lower-order ones (and new ensuing behavior). These conjunctions indicate a shrinking of the parametric space in which the postulated simple behavior will occur. In terms of our prior beliefs about bureaucratic behavior, one may feel confident in asserting, say, that individual choice rules are linear, or that they are deterministic, or the like. But would many scholars in this field confidently assert all five conditions? We doubt it.

Bounded Rationality: Individual and Organizational

A second problem of Model II concerns the relation between individual and organizational constraints on rationality. Organization theorists have often moved "rather cavalierly from theories of individual cognition and choice to theories of organizational cognition and choice" (March and Shapira 1982, 11). This tradition has sometimes been carried to the point of anthropomorphizing organizations, positing that organizational constraints on information processing simply mirror individual constraints.

This quick passage from individual to organizational limits on rationality is evident in *Essence of Decision*: "Simon and the Carnegie School focus on the bounded character of human capabilities. Firms are physically unable to possess full information, generate all alternatives" (Allison 1971, 174). Again, "The physical and psychological limits of man's capacity as alternative generator, information processor, and problem solver constrain the decision-making processes of individuals and organizations" (p. 71).[18]

Undoubtedly the bounded rationality of single decision makers constrains the information-processing capacities of agencies in some fashion. However, we maintain that the relation between the two is more complex than is portrayed by either Model II, in particular, or much of organization theory, more generally. For normative reasons—and because the literature has focused heavily on the tendency of institutions to be dumber than their members (via, e.g., conformity pressures)—we are especially interested in how organizations can be *smarter* than the individuals who compose them. We therefore consider three reasons why organizations can process information more effectively than individuals can.

From Serial to Parallel Information Processing. In a neglected passage of *Organizations*, March and Simon note an important difference between individual and collective cognition: "The individual possesses only a single focus of attention, hence can only deal with one aspect of the problem at a time. In organizations, however, there is no limit to the number of attention centers that can be allocated to the parts of a problem" (1958, 193). They go on to note the advantages of parallel processing. Clearly, if one large problem can be decomposed into many subproblems, an agency can greatly speed up its information processing by assigning a different official to each subproblem.[19] Of course, there is no guarantee that any given division of labor will be an effective decomposition. The trick, as many students of organizational design have noted, is to figure out a scheme of specialization that "carves nature at the joints" (see Gulick 1937; March and Simon 1958; Simon 1947; Simon, Smithburg, and Thompson 1950). When this can be done, the serial constraint can be substantially eased.

This organizational advantage is obvious. Just mentioning the phrase, *division of labor*, helps us recognize an organization's advantage over an individual. In the simplest model of problem decomposition (an additive one) information processing is essentially the same as performing a physical task, like Frederick Taylor's ([1911] 1947) pig iron loading. Obviously, the more people, the more pig iron is loaded or the faster information is processed. Otherwise, why bother to hire the additional people? Yet in the organizational decision theory literature, this obvious point is sometimes overlooked.

A second, more subtle advantage of specialization is that it enables decision makers to become experts in their domains. Becoming an expert means relying less on ordinary folk heuristics, with their attendant biases and more on scientifically based inferences, with their lower rates of error (Hogarth 1987; Nisbett and Ross 1980). As Nisbett and Ross replied to a colleague who, having read a draft of *Human Inference*, asked, "If we're so dumb, how come we made it to the moon?"—

> Humans did not "make it to the moon" by trusting the availability and representativeness heuristics or by relying on the vagaries of informal data collection and interpretation. On the contrary, these triumphs were achieved by the use of formal research methodology and normative principles of scientific inference. Furthermore. . ., no single person could have solved all the problems involved in such necessarily collective efforts as space exploration. Getting to the moon was a joint project, if not of idiot savants, at least of savants whose individual areas of expertise were extremely limited- . . . . Finally, those savants included people who believed that redheads are hot-tempered, and who at the advice of an acquaintance's brother-in-law bought their last car at the cocktail-party. (1980, 250)

Reliable Systems, Unreliable Components. In his book on the Polaris missile system, Sapolsky (1972) observed that several teams worked independently to develop a key component. The Navy's Special Projects Office deliberately established this redundant structure to increase the odds that the essential component would be ready by a specified date. By doing so, the Special Projects Office loosened the connection between the reliability of an organizational subunit (one of the problem solving teams) and the larger organization—in contrast to the message of Model II.

More generally, a properly designed agency can display large disparities between the reliability of different organizational levels (Bendor 1985; Grofman, Owen, and Feld 1983; Landau 1969). Consider a representation of the Polaris project. There are n teams trying to develop the same component. The probability that any one of the teams will succeed in the specified time is p, where $0 < p < 1$. If the performance of the teams is independent, the probability that the organization will succeed equals p(at least one of the teams succeeds), which equals $1 - [(1 - p)^n]$. This probability increases steadily toward one as n increases, so long as p exceeds zero. Thus, in this simple setting, the performance of subunits and units need not be closely connected.

From Tunnel Vision to Innovation. Model II's image of organizational change is of sluggish entities dominated by a single way of thinking. Yet modern bureaucracies are often populated by different kinds of professionals with distinctive mindsets. What is commonplace to one professional culture may be dramatically new—even bizarre—when introduced to an agency dominated by another profession. Observe, for example, the clash between civil engineers and environmental analysts in the Corps of Engineers (S. Taylor 1984). The questions raised by new environmental analysts about water projects, though alien to the engineers and the Corps, were standard in the community of environmental analysts. Or consider the idea of vouchers for schools. To a professional with a background in educational administration, vouchers are a dramatic departure from the status quo. But to an economist specializing in education policy, they are an obvious extension of the basic principle of market competition. In both examples, innovation was based on straightforward applications of professional expertise; heroic mental efforts were not required.

Scholars typically understand the main lesson of Model II to be that routines and standardized scenarios constrain and rigidify organizational behavior (e.g., Art 1973, 476–79; Gallucci 1975, 145; Jefferies 1977, 231–32; Krasner 1972, 169–75; Levy 1986; Peterson 1976, 113; Scott 1981, 6; Thompson 1980, 27; Williamson 1979, 140). (For a partial exception to this pattern, see Posen 1984, 46–47.) True, Allison occasionally mentions the possibility of organizational innovation and at times portrays standard operating procedures as efficient ways of handling standard problems.[20] But most of chapter 3's theoretical exposition emphasizes the negative, constraining effects of organizational routines. And this pattern is strikingly reinforced in chapter 4's empirical application of Model II to the missile crisis. Virtually without exception, the episodes covered there describe how the permanent bureaucracies of both countries fouled things up. Toward the end of chapter 4, Allison pointedly asks, "Were the organizations on top of which the President was trying to sit going to drag the country over the nuclear cliff in spite of all his efforts?" (1971, 141). The heroes of this chapter are clearly the unbureaucratic president and his advisors. Therefore, the common interpretation that Model II's main lesson concerns the rigidifying, maladaptive effects of bureaucracy is well founded.

This is an irony of intellectual history. As Allison noted, the central ideas of Model II are rooted in the Carnegie School. Yet a founding volume of that tradition, Simon's *Administrative Behavior*, views routines far more positively:

> Habit performs an extremely important task in purposive behavior, for it permits similar stimuli or situations to be met with similar responses or reactions, without the need for a conscious rethinking of the decision to bring about the proper action. Habit permits attention to be devoted to the novel aspects of a situation requiring decision. A large part of the training that goes to make a championship football team, crew, army battalion, or fire company is devoted to developing habitual responses that will permit immediate reactions to rapidly changing situations.
>
> Habit, like memory, has an artificial organization counterpart, which has been termed by Stene "organization routine." (1947, 88)[21]

Indeed, Simon goes much further than this in praising the general role of organizations in amplifying individual rationality:

> It is impossible for the behavior of a single, isolated individual to reach any high degree of rationality. The number of alternatives he must explore is so great, the information he would need to evaluate them so vast that even an approximation to objective rationality is hard to conceive. . . . One function that organization performs is to place the organization members in a psychological environment that will adapt their decisions to the organization objectives, and will provide them with the information needed to make these decision correctly. . . . In the course of this discussion it will begin to appear that organization permits the individual to approach reasonably near to objective rationality. (pp. 79–80).

Hence, chapter 4 of *Essence of Decision* inverts Simon's view. Instead of organizations boosting individual rationality, the bureaucracy's rigid conduct nearly nullified Kennedy's best efforts.[22]

Why do Simon's and Allison's books assess standard operating procedures in particular and organizations in general so differently? We believe that different baselines of evaluation are at work here. In *Administrative Behavior*, Simon began with the premise that *individuals* are boundedly rational. With this as a starting point, it is not surprising that he emphasized the enabling aspects of standard operating procedures and specialization. Given the real limits on individual cognition, technically demanding projects such as going to the moon can be handled only by collective endeavor. In contrast, *Essence of Decision* begins with Model I, which presumes perfectly rational individuals. Whether Allison intended unboundedly rational decision makers to be the standard for evaluating Model II actors is not completely clear; but the book's structure and the frequent use of Model I as an analytical benchmark suggest that this was the implicit standard.[23] With that baseline, it is not surprising that Model II organizations, staffed by boundedly rational individuals, appear more the problem than the solution. But we would ask, How well could the United States have gathered information about the Soviet missiles in Cuba, conducted the blockade, and carried President Kennedy's message to the world had there been *no* routines to rely on? How effective would the U.S. response been if it had had to invent all procedures on the spot? Had Allison disentangled the different effects of bounded rationality and organizational action by inserting a model of a unitary-actor-with-imperfect-rationality in between Models I and II, his evaluation of the *net* effects of organization—holding constant the degree of individual rationality—might have changed considerably.

ANALYSIS OF MODEL III

Allison's central argument in Model III is easily stated: "The name of the game is politics: bargaining along regularized circuits among players positioned hierarchically within the government. Government behavior can thus be understood according to a third conceptual model, not as organizational outputs but as results of these bargaining games" (1971, 144). It proved difficult, however, to build a clear and coherent model around this claim. Some of the fault lies with the literature on which Allison had to rely. For the rational actor model, he could draw on well-specified, axiomatically constructed theories. The organizational process model is based on works that advance propositions that are thematically (though not deductively) connected. In contrast, the literature on bureaucratic politics is more discursive and far less explicitly theoretical. Allison himself remarked on this difficulty: "Unfortunately, the interests of these analysts have not led them to invest much labor in squeezing their insights into propositions. Consequently, the paradigm . . . can be no more than a tentative formalization" (p. 147). Even his tentative formalization, however, attracted far more critical attention from reviewers of *Essence of Decision* than either of his other models.

Criticisms of Model III can be grouped into four categories. First, many critics argued that Model III misconstrues the nature of executive branch policymaking. In particular, we will argue, the central claim (that policymaking necessarily involves bargaining among executive branch decision makers) must be questioned. Second, we will point out that Model III said surprisingly little about the fact that executive branch policymaking takes place within a *hierarchy*. Hierarchy's impact on policymaking is something that both Allison and many of his critics overlooked. Third, we will argue (as did several critics) that Model III is the least precisely formulated of Allison's approaches. Not only are its fundamental assumptions much less clear than those of Models I and II, but many of the propositions seem to be ad hoc observations not derived in any rigorous sense from the assumptions. Finally, while many critics questioned Model III's premise of policymaking-as-bargaining and some pointed out that Model III's ambiguities raise questions about its status as a model, few critics made an argument about Model III that we think is equally important, namely, that the model is simply too complex. There are so many different assumptions, variables, and relationships in Model III that it is almost impossible to determine the role and impact of any one of them. For Model III to be useful for systematic analysis, it must be simplified considerably.

Does Policymaking in the Executive Branch Proceed Via Bargaining?

It is not hard to distill from the bureaucratic politics literature the claim that policymaking in the executive branch proceeds via bargaining. Yet this claim is not a completely accurate reading of the literature. And whatever the literature says, the claim turns out to be vulnerable to several different kinds of criticisms. We will examine these difficulties by posing a series of questions about the properties of Model III.

Do Executive Branch Decision Makers Always Have Different Goals? Given our typology of models of policymaking in Figure 1, the assumptions of Model III would clearly seem to place it on row 3: there are many decision makers and they have conflicting goals. However, row 2 of the typology raises the possibility that these multiple decision makers may sometimes have the *same* goals. Indeed, Krasner suggests that considerable similarity in policymakers' views should be expected: "The President chooses most of the important players and sets the rules. He selects the men who head the large bureaucracies. These individuals must share his values. Certainly they identify with his beliefs to a greater extent than would a randomly chosen group of candidates" (1972, 166).

Moreover, Art (1973) argues that shared "mindsets" often influence foreign policymaking. He notes, for example, that in the decisions to intervene in Korea and the Dominican Republic, "there was unanimity on the need to use military force, once the principals involved realized that nothing short of that would achieve American objectives" and that for Cuba and Vietnam, "the need to use force was questioned by one or two principal advisors; but they were clearly out of the mainstream of the President's thinking and his advisors' outlooks" (p. 481). The implication of Art's point is clear: "If shared images dominate senior players' outlooks and if they are truly *shared*, then what is the merit in asserting that governmental actions are the resultants of pulling, hauling, and bargaining?" (p. 476). To the extent that Model III assumes that actors have conflicting goals, these examples suggest some clear limits to the model's empirical domain.

Does Conflict Imply That Policymakers Have Different Goals? When executive branch conflict does occur, understanding its causes requires that we distinguish between differences over goals and differences over beliefs about how to achieve the goals. The relative absence of conflict in Art's four cases might suggest that the key decision makers did share objectives. However, even if actors' goals are identical, they may disagree about how to achieve those ends, and these conflicting beliefs might foment arguments. While Allison does mention the importance of differing beliefs, he does not adequately distinguish between conflicts caused by differing beliefs and conflicts caused by differing goals.

What kind of policymaking should we expect among actors with conflicting beliefs? Thompson and Tuden (1959) suggested that when there is agreement on beliefs about causation but disagreement on goals, then bargaining will be observed, as in Model III. But where there is agreement on goals but disagreement on beliefs, they suggest that "collegial" judgment will prevail. Collegial problem solving involves efforts to ascertain the justification for differing beliefs and to change the beliefs of other actors. If agreement on beliefs remains elusive, more overtly political activities might occur, though it seems likely that actors would try these only as a last resort. Why yield something by bargaining when one might persuade others of the empirical correctness of one's position? When stakes are high, outcomes uncertain, and beliefs deeply held, debates over how to reach a common end may become rancorous; passionate disagreements need not indicate goal conflict. But such debates differ significantly from those caused by conflicting objectives.

As an empirical question, Did President Kennedy and the various members of the ExCom have conflicting goals? Decisions were certainly made in a partisan context. Recall the criticisms by congressional Republicans and the administration's nervousness about the upcoming congressional elections. But by Allison's own account this was more a matter between the president and his critics *outside* the administration than among ExCom members (1971, 187–200). Certainly, administration officials argued over which action to take; but these disagreements could have stemmed mainly from honest differences in beliefs about the effects of various options. While Allison criticizes Sorensen (1963) and Schlesinger (1965) for presenting "the efforts of the ExCom in the Cuban missile crisis as essentially rational deliberation among a unified group of equals" (p. 146), it is not apparent from Allison's own account whether differences in personal and institutional goals (hence, "politics") or differences in beliefs (hence, "collegial," and possibly "rational," decision making) were more important in the ExCom debate.[24]

Why Would a President Bargain with Other Executive Branch Officials? One of Model III's great ambiguities concerns the nature of the president's relations with other officials in his administration. At one point, for example, Allison approvingly comments on Neustadt's picture of the president's role in policymaking: "Sometimes Neustadt's 'President-in-sneakers' is the central political gamesman. Sometimes the President merely observes the pulling and hauling among various groups within the government. Sometimes the President is the target of the tactics of a bureaucratic group" (1971, 158). Later, Allison also states, "Where an outcome was for the most part the triumph of an individual (e.g., the President) or group (e.g., the President's men or a cabal) this model attempts to specify the details of the game that made the victory possible" (p. 173). It would seem, then, that the extent of the president's involvement in policymaking—and his ultimate success—can vary from case to case.

Yet the central thrust of Model III is not this bland point that the president's influence varies across issues. Instead, it is the much bolder hypothesis that in order to make policy the president must bargain with members of his own administration. As Allison himself put it, "The primary source of the paradigm is the model implicit in Neustadt's work, though his concentration on Presidential action has been generalized to a concern with action as a resultant of political bargaining among a number of independent players, the President being only a 'superpower' among many lesser but considerable powers" (1971, 162). This raises an obvious question: Since the president has personally appointed the top officials in his administration and can dismiss them at any time and since he has substantial formal authority (especially on foreign policy issues) to order them to do what he wants, why must the president bargain with them?

The literature on which Allison based *Essence of Decision* certainly does not lend unqualified support to the idea that superiors will bargain with their subordinates. For example, March and Simon (cited by Allison for Model II) note that such bargaining has some "potentially disruptive consequences":

> Bargaining almost necessarily places strains on the status and power systems in the organization. If those who are formally more powerful prevail, this results in a more forceful perception of status and power differences in the organization (generally dysfunctional in our culture). If they do not prevail, their position is weakened. Furthermore, bargaining acknowledges and legitimizes heterogeneity of goals in the organization. Such a legitimation removes a possible technique of control available to the organizational hierarchy. (1958, 131).

Thus, bargaining is something the president would seek to avoid, if at all possible. The question is whether he can avoid it.

Model III, of course, is based on the bureaucratic politics literature, and this literature reveals ample support for an argument that the president often *can* avoid having to bargain with subordinates. For example, Huntington's *Common Defense* has a substantial section analyzing when policymaking is *legislative* in character and when it is *executive* (1961, 146–59). It is interesting to note that Allison quotes a passage from this section (*Essence of Decision*, 156). Indeed, he even cautions that Huntington's analysis "overemphasizes participant equality as opposed to the hierarchy that structures the game" (p. 162). Hence, it is all the more puzzling that Allison nowhere actually *uses* Huntington's arguments. Instead, he just assumes that executive branch policymaking will always be legislative in nature; the possibility of executive policymaking is simply ignored.[25]

Thus, if presidential power is a *variable*, as Huntington suggests, when are presidents powerful and when not? The literatures on bureaucratic politics and organization theory reveal two basic reasons why a president may sometimes be in a disadvantageous position vis-à-vis members of his own administration. The first is that subordinates may have sources of political support outside the executive branch. The second turns on Weber's classic argument about informational asymmetries between a superior and his subordinates. Allison's presentation of Model III does mention these two sources of power (1971, 168–69); but his model does little to explore the role of either factor in policymaking.

Do Subordinates Have Political Support outside the Executive Branch? While the president has much more formal authority than his appointees, his authority in the larger political system is far from absolute. Because Congress has an important constitutional role in policymaking, agencies in the executive branch find it useful to cultivate support in Congress. While the president can often order his political appointees and their respective bureaucracies to do his bidding, they may be able to hurt him politically if they disagree with his choices and make their disagreements known to outside supporters. Hence, the president may end up bargaining with subordinates not because they are intrinsically powerful but because their outside supporters can make life difficult for him. Thus even though the most obvious manifestations of bureaucratic politics may take place inside the executive branch, what is driving the politics may be happening outside.[26]

Of course, when the most important actors in the environment support the president, he need not bargain with any subordinates who are allied with them. Moreover, even if influential decision makers, whether inside or outside the executive branch, have diverse views and even if some of them are hostile to the president, the president may still be left relatively free to select who will be his allies. As Art suggests, "When senior executive players are split on their policy stances, the President, by virtue of the division, has considerable leeway to choose that which he wishes to do, or that which he thinks he ought to do, or that which he reasons he must do" (1973, 475). It is primarily when most of the environment is hostile and the president lacks countervailing support that he must bargain with his nominal subordinates. In sum, support outside the executive branch does not always mean influence inside it.

Do the President's Subordinates Have Informational Advantages? Even a subordinate who lacks support outside the executive branch may still enjoy an informational advantage over the president. Nonetheless, it is not obvious that bargaining will be the empirical manifestation of informationally based influence. Three categories of informational asymmetries can be distinguished. For a president to exercise his authority over the executive branch, he must become aware of what problem needs to be addressed, decide what to do about it, and have his choice implemented. For each of these three tasks, subordinates may have more information or expertise than the president.[27] This puts them in a position where they can manipulate what problems he is aware of, what advice he receives, and how his final choice is implemented.

However, the standard works on bureaucratic politics in the era of *Essence of Decision* (e.g., Downs 1967; Halperin 1974; Rourke 1969; Tullock 1965), as well as the more recent literature on incentives (e.g., Bendor, Taylor, and Van Gaalen 1987), suggest that there are ways the president can alleviate these informational problems. A wide-ranging communication network can alert him to a problem; and if he then attends closely to the problem, he can learn a good deal about it, thereby reducing his subordinates' informationally based influence. And once he knows what he is doing, he is not without weapons, as Krasner observed: "The Chief Executive involves himself in those areas which he determines to be important. When the President does devote time and attention to an issue, he can compel the bureaucracy to present him with alternatives. . . . Even when Presidential attention is totally absent, bureaus are sensitive to his values. Policies which violate Presidential objectives may bring Presidential wrath" (1972, 168–69). Kohl (1975) provides systematic support for this argument: in an empirical test of several models of president–subordinate relationships, the attention of the president and his top aides and the coherence of their ideas emerged as key variables. Bureaucratic politics flourished largely when the president and aides paid little attention to an issue or lacked clear policy preferences about it.

Finally, even if informational asymmetries persist, the subordinates' influence depends on the president not realizing that he is ignorant. The subordinates' influence is maximized when the president is unaware of what constitutes good advice or that a problem exists or that implementation should be monitored. Even here, however, we think their influence would not make itself felt through *bargaining*. If the president does not realize that he is ignorant, the very fact that his subordinates try to bargain with him should alert him that something odd is going on. To influence the president, the subordinates would have to use their informational advantage in more subtle ways.

Was U.S. Policy in the Cuban Missile Crisis Determined by Bargaining? While our purpose is largely theoretical, it is of interest to ask whether Model III accurately describes U.S. policymaking during the missile crisis. It seems to us that the most obvious manifestations of politics—coalition formation, bargaining, logrolling, splitting the difference, leaks to the press—were largely absent from the ExCom deliberations.[28] Instead, our reading of *Essence of Decision* is that policymaking revolved around President Kennedy and that decision making lay largely in his hands. Allison's own evidence reveals that the participants themselves believed that one person was in charge. Recall, for example, his observation that Dean Acheson, emerging from an unsuccessful effort to convince the president to approve an air strike, "left with no question in his mind about where the buck stopped" (1971, 207). Allison even quotes Robert Kennedy's observation, as the crisis reached its peak, "It was now up to one single man. No committee was going to make this decision" (p. 208).

Moreover, a key tenet of Model III is that policy outcomes are *resultants*—unintended by any one actor—that emerge out of the dynamics of bargaining. But if policy outcomes are unintended resultants and if Model III accurately describes the ExCom deliberations, one must then conclude that the choice of the blockade was unintended by President Kennedy. But Allison provides no evidence that the president preferred some other option. On the contrary, his story suggests that the blockade was the option most favored by the president and his closest associates; it was not chosen by the president as part of a bargain or political compromise with other ExCom members.[29]

A Model of Bureaucratic Politics Needs a Model of Hierarchy

Model III was intended to synthesize the bureaucratic politics studies of Hilsman, Huntington, Neustadt, Schilling, and others. What is odd about the model—a model of politics in a hierarchy—is that it says almost nothing about how the hierarchy affects the politics. References are made to "players in positions," "action-channels," and "the rules of the game" (1971, 164–66, 169–71); but it is almost impossible to infer from Allison's brief mention of these variables how they are expected to affect outcomes. This neglect of the impact of hierarchy is surprising, since Schilling (1962)—whom Allison discusses at length (pp. 154–56)—offered some useful insights about hierarchy's impact on policymaking. In his study of the 1950 defense budget, Schilling argued that the shape of the military hierarchy affected who handled which issues. Schilling described several post–World War II plans to reorganize the defense establishment, noting claims that particular plans would reduce the interservice rivalry over the military doctrines justifying particular allocations of missions and budgets. One proposal was to reorganize the land, sea, and air forces into unified functional commands: one was for conducting limited war, one was for all-out war, and one was for continental defense. Such reshuffling, Schilling argued, would not end the key conflicts:

> Some of the issues which had formerly been subject to inter-service argument and determination would become matters for intracommand argument and determination (for example, the Air Force–Navy dispute over the relative merits of sea as compared to land-launched vehicles for the delivery of a nuclear strike would now take place within the confines of the All-Out War Command). Conversely, other issues formerly subjects of intraservice conflict would now become matters of intercommand dispute (for example, the argument within the Air Force over the allocation of resources between tactical and strategic air forces would now be joined between the All-Out War Command and the Limited War Command). (p. 228)

This rearrangement of the issues could affect how the conflicts were settled. As Schilling put it, "What reorganization would change, and it would be a consequential change, would be the political conditions under which these issues would be argued and resolved" (1962, 228). For example, since an organization's design thrusts some issues to the top while pushing others down and since top decision makers care more about some issues than others, different choices might be made in different structures.

Only recently have Schilling's ideas been used to develop a more explicit theory of hierarchy's impact on policymaking. The essential ideas in one set of studies, for example, are that policymaking involves making comparisons (of pieces of information, of policy options, or of proposals for implementation), and that an organization's structure affects who compares what with what, so that different structures can produce different policy outcomes (Hammond 1986, n.d.; Hammond and Thomas 1989). It is well known that a legislature's agenda, which specifies the sequence of voting on amendments, can greatly influence the outcome. An organization's formal structure can, via its impact on the sequence of decisions, have a similar impact on which policies are ultimately adopted.[30]

Allison apparently did judge that hierarchy was relatively unimportant in the missile crisis. He observed at one point, for example, that the ExCom "functioned with minimal reliance on the standard channels down into the second or third levels of the government, causing no little pain to the players left out of the action" (1971, 215). Hence, it may have seemed unnecessary to explore the impact of hierarchy. But what is at issue, we would argue, is whether the hierarchy's *particular configuration* influenced, for example, the CIA's information processing (which Model II treats as an apolitical concern) or the particular policy proposals advanced by the bureaucratic chiefs represented on the ExCom. Without considerably more attention to hierarchy as a variable, Allison could not have determined whether the bureaucratic hierarchy actually had a negligible impact on policymaking.

Logical Problems with Model III's Propositions

Thus far, our argument has been that Model III's conceptual development is inadequate. Some of the model's premises are ambiguous, and it is difficult to determine from Allison's discussion when bargaining will characterize policymaking and who will have to bargain with whom about what. These issues—central to a theory of bureaucratic politics—are left unresolved by Model III. Model III's propositions, which define how it should be applied to data, are no more satisfactory.

A *proposition*, in Allison's usage, is an empirically testable hypothesis that can shed light on the validity of the underlying theory. But it is difficult to know what to conclude from a careful examination of some of his propositions. For example, consider the proposition, "Where you stand depends on where you sit" (1971, 176). Several critiques noted that this proposition contradicts other aspects of Model III (Art 1973, 472–73; Ball 1974, 77; Caldwell 1977, 94; Krasner 1972, 165); Allison himself mentions many other influences, besides bureaucratic position, on the stances of participants. Moreover, empirical evaluation of the proposition is difficult, since, as these critics observed, some key participants do not "sit" anywhere. And even where the proposition can be empirically scrutinized and is found to lack support (as when, in the missile crisis, the secretary of defense initially took a relatively dovish stand despite the hawkish views of many of his uniformed subordinates), it is not clear what conclusions we should draw about Model III more generally.

In fact, some of the propositions seem to be only ad hoc generalizations lacking clear derivation from the underlying model.[31] For example, it is not at all apparent how the claim "In a nuclear crisis, the central decisions will be hammered out *not* in the formal forums, e.g., the National Security Council, but rather by an *ad hoc* group that includes the

President, the heads of the major organizations involved, plus individuals in whom the President has special confidence" (1971, 180) stems from the central argument of Model III, namely, that policymaking proceeds via bargaining. Hence, even if the proposition were found to have empirical support, one's judgments about the model from which it was (supposedly) derived would be largely unchanged. Such a proposition would seem to have little scientific value.

Model III Is Too Complex

Finally, some of Model III's problems stem from its extraordinary complexity. Whereas Allison's formulation of the unitary rational actor model is too "thin," Model III is simply too "thick." It incorporates so many variables that it is an analytical kitchen sink. Nothing of any possible relevance appears to be excluded, as the following passage illustrates:

> A Secretary of State's resolution of these conflicts depends not only upon the position, but also upon the player who occupies it. For players are also people; men's metabolisms differ. The hard core of the bureaucratic politics mix is personality. How each man manages to stand the heat in *his* kitchen, each player's basic operating style, and the complementarity or contradiction among personalities and styles in the inner circles are irreducible pieces of the policy blend. Then too, each person comes to his position with baggage in tow. His bags include sensitivities to certain issues, commitments to various projects, and personal standing with and debts to groups in the society. (1971, 166; emphasis original)

Indeed, after reading chapters 5 and 6 carefully, one is hard pressed to determine what, if anything, escapes the purview of Model III.

An analyst must make some hard choices about what variables a theory should include and what it should exclude. It is often argued that there is a trade-off between explanatory richness (ability to explain one case well) and theoretical generalizability (ability to explain many cases well). Thus, one may conclude that a complex theory, like Model III, can have considerable value. But the nature of this trade-off has been oversimplified: it is possible to include so many variables that the theory does not explain even one case very well. A model that is as complicated as the phenomena it represents is of little use.

This is the trouble with Model III: the relations of so many variables in Model III are left so obscure that it is difficult to use the model even to explain the Cuban missile crisis. In our view, it would have been advisable to pay closer theoretical attention to a smaller number of variables—perhaps just the impact of the domestic political environment and the nature of hierarchical relationships inside the executive branch (with a focus on authority, expertise, and external political support). In general, a model that includes everything explains nothing. If it does not simplify, it cannot explain.[32]

CONCLUSION

Allison's classic book showed an entire generation of scholars and students how to study bureaucracy's role in foreign policy making, and its role in making public policy more generally. *Essence of Decision* made a persuasive case for the use of formal reasoning, for the development of alternative models to explain an important event, for the derivation of testable propositions from the models, and for the testing of the propositions. This approach to bureaucracy was relatively unfamiliar to students of bureaucracy in the 1960s, and Allison's message remains important to this day. It is the rare scholar whose research on organizations and bureaucratic politics has been uninfluenced by the approach advanced by *Essence of Decision*. For these reasons, it legitimately remains one of the most frequently cited studies in the literatures on bureaucracy, foreign policy, and governmental policymaking. If its readers come away with just these methodological lessons, it will remain an important book.

But *Essence of Decision* is more than an exercise in methodology. A very ambitious work, it is simultaneously a treatise on models, a historical study of the Cuban missile crisis, and a heuristic guide to explaining other major foreign policy crises. We admire its reach. But in trying to do so much, it had to accept certain trade-offs.[33] Most importantly, there is an inevitable tension between attempting to explain a particular event (a task characteristic of historians) and attempting to construct models (a job more characteristic of social scientists).[34] The demands of specialization, of allocating one's limited time either to investigating particular historical facts or to developing the mathematical expertise needed in model building, imply that this trade-off is a difficult one. Moreover, these opportunity costs apply to readers as well as authors. Scholars who read *Essence of Decision* primarily to learn more about the missile crisis in particular or cold war diplomacy in general have typically invested much time studying important past foreign policy events. Consequently, they are not trained to read formal models. Thus, Allison would not only have incurred substantial opportunity costs had he tried to transform his analyses into genuine models, but he might also have lost much of his audience. It is, in short, exceedingly difficult for a single volume to be both a work on social science theories and a historical study. A fair assessment of the book must recognize these trade-offs and its ensuing compromises. Nevertheless, we are forced to conclude that the price paid by *Essence of Decision* on the theoretical dimension was that due to its attention to historical detail, it paid insufficient attention to the internal logic of the models.

Thus, a close examination of these models shows that they are much less rigorously formulated than is generally recognized, that the derivations are in some cases wrong and in others do not follow from the models, and that the meaning of the empirical tests is often quite ambiguous. Hence, the continued reference to, and use of, *Essence of Decision* has a rather troubling character. Aside from its important methodological points, what scholars and students have been learning from the models may lack foundation and, in some cases, may simply be incorrect. We conclude by emphasizing five of these problems.

First, modeling generally begins with some hunches of the theorist—or with the claims of the academic literature—about how the world works. The model should then reflect these judgments. But none of Allison's models are, in our view, sufficiently accurate renditions of the literatures that he himself cites in justification for the models. The traditional literature on war, crisis, and international politics has emphasized uncertainty and state-versus-state gaming. Model I almost ignores these properties. The organizational literature of Simon and March portrayed standard operating procedures and routines as *enabling* choice and action. Model II depicts them as *constraining* choice and action. The understanding of politics-in-hierarchies, available in the bureaucratic politics literature by the late 1960s, is not accurately reflected by Model III's analysis of bureaucratic bargaining. The role of hierarchy, which presumably structures the entire game of bureaucratic politics, is neglected—and the critical interplay between authority and expertise in a bureaucracy, a major topic of discussion in organization theory since Weber, receives almost no attention at all.

Second, in laying out his three models of policymaking, Allison has generally been credited with advancing the important point that intellectual progress comes from formulating, testing, and evaluating alternative models. Yet his presentation of Model I suggests that he saw it, at least in part, as something to be set up in order to be knocked down. This may explain Model I's inadequate development, even given the relatively crude state of the art of game theory and rational choice models when Allison was writing. Nonetheless, one cannot seriously evaluate the relative performance of two new models (II and III) if the baseline model is almost destined to fail. For this reason, what is often taken to be a major lesson of *Essence of Decision*—that rational choice explanations of policymaking are weaker than those of Models II or III—lacks foundation. Whatever their ultimate worth, rational choice models were not given a fair test.

Third, Allison's overall approach—the development of explicit models of policymaking—was based on the view that clarifying one's key assumptions is analytically helpful. Yet the assumptions of Models II and III were sufficiently ambiguous that it is difficult to discern the models' defining properties. Hence, it is unclear whether Models II and III belong to separate classes of theories or not.

Fourth, one of the purposes of developing an explicit model is rigorously to derive the logical implications of one's fundamental assumptions. Yet for neither of the two models (II and III) for which *Essence of Decision* is best known are the propositions rigorously derived. The general proposition to be drawn from Model II—that simple rules lead to simple, predictable behavior—is almost surely wrong; and Model III is so complicated that virtually no propositions can be rigorously derived from it at all.

Fifth, if propositions are not rigorously derived from a model yet receive some evidential support, it is difficult to know what one should learn from the empirical corroboration. We may learn something about the propositions; but lacking any logical relationship between them and the model, the empirical test teaches us little about the model or the hunch that originally generated it.

Some of the inadequacies of Allison's models stem merely from the fact that there has been considerable progress in the understanding of game theory and rational choice models, in the appreciation of the role of uncertainty and incomplete information, and in development of insights about organizations, hierarchy, and bureaucratic politics. Indeed, we have been at pains to point out what literature was available to Allison—and what was not—when he wrote his book. Yet many of the flaws we have pointed out in *Essence of Decision* do not stem from advances in these fields: they were there from the beginning, and a reasonable assessment of the book must recognize this fact. Since the book still has considerable currency in political science and in a wide range of other disciplines, continued use is thus likely to lead to the widespread perpetuation of major misunderstandings about the nature of bureaucracy and governmental policymaking. If the academic community is not made aware of these flaws, reliance on Allison's models is as likely to lead to error as it is to inform. Nonetheless, *Essence of Decision* richly deserved its two decades of intellectual prominence. The fact that the study of bureaucracy, organization, and foreign policy making has gone beyond it detracts in no way from its remarkable accomplishments.

Notes

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1. Perlmutter (1974) distinguished three different models, as did Snyder and Diesing (1977) and Steinbruner (1974). Kurth (1971) analyzed five models, Kohl (1975) six, while Caldwell (1977) distinguished 12 models! Allison himself cited Steinbruner's contribution of additional models and noted that "a number of others are clearly possible" (1971, 255; see also pp. 276–77).

2. E.g., in the 1990 *Social Science Citation Index*, *Essence of Decision* and Allison's 1969 article were cited 84 times in 58 different journals.

3. Though classical decision theory emphasized prospective uncertainty, retrospective uncertainty is also prevalent in crises: Did the enemy just launch a first strike, or are we picking up a flock of geese on our radar screen? This example also points up the connection between the two types of uncertainty: if one is unsure what one's opponent has done,

one will also probably be unsure how he will react to one's own move.

4. For just one example (which could have initiated a nuclear exchange), recall the U-2 that blundered into Soviet air space during the height of the Cuban missile crisis. As President Kennedy was reported to have remarked about this incident, "There is always some son-of-a-bitch who doesn't get the word" (quoted in Allison 1971, 141).

5. Even if individual attention were unconstrained, costly communication would mandate this issue. As Simon (1973) suggests, however, attention is often a major bottleneck.

6. Given this assignment, Model I should not be seen as the only rational choice model in *Essence of Decision*.

7. There is a tendency to jump to such claims, particularly regarding unitary rational actor models; but we shall argue that these claims oversimplify the models' implicational richness.

8. Ball argues that Model III "is the model closest to Allison's own heart," citing a seminar discussion at Harvard in October 1972, in which "Allison himself made this quite clear" (1974, 76).

9. Cognitive analyses (e.g., Steinbruner 1974) recognize that decision makers may sometimes fail to resolve intrapersonal conflict in a consistent manner.

10. In redundancy theory, one often analyzes the optimal amount of duplicate men and materiel to achieve a task when implementation is uncertain. These analyses are facilitated by assuming a single decision maker whose objectives are to be maximized.

11. Indeed, Blight and Welch (1989) present ample evidence that even latter-day *Soviet* analysts disagree about Khrushchev's motives.

12. Allison does challenge this notion, but only in discussing Model III in respect to bargaining *inside* governments.

13. Mixed strategies are covered in game theory texts going back to Luce and Raiffa 1957, chap. 4; see also Schelling 1960. Note that in the inspection game and similar two-player games, the mixed strategy equilibrium is replaced by a pure strategy equilibrium if the players move sequentially. The indeterminacy of a mixed strategy equilibrium does require some exogenously assumed uncertainty (i.e., the imperfect information occasioned by simultaneous play).

14. Clearly, the optimal outcome for the United States is to deceive the USSR, convincing the Soviets that we will inspect closely, but not to do so. A similar point about deception applies to the Soviets. Neither optimal outcome involves a mixed strategy. However, this does not invalidate the text's point, since neither outcome is an equilibrium.

15. For example, Cyert and March describe the general procedure—"Use simple rules"—as one of the "three basic principles" of choice (1963, 102). And certainly, the best-known rule in this literature, *satisficing* (searching for alternatives until finding one that exceeds a specified threshold) is a very simple procedure (Simon 1957, 204–5, 252–53).

16. March and Simon, who pioneered the metaphor of organizational behavior as governed by computer programs, recognized that behavioral flexibility could arise from a program's sensitivity to inputs: "The term 'program' is not intended to connote complete rigidity. The content of the program may be adaptive to a large number of characteristics of the stimulus that initiates it. Even in the simple case of the fire gong, the responses depend on the location of the alarm" (1958, 142). Again, "At the [programmed] limit, an environmental stimulus may evoke immediately from the organization *a highly complex* and organized set of responses" (p. 141, emphasis added).

17. What follows is therefore a conservative analysis that underestimates the complexity of the armed services' *rules* for research-and-development and procurement.

18. This view of Allison's appears to have influenced other international relations scholars. For example, Keohane wrote that "If individuals typically satisfice rather than maximize, all the more so do governments and other large organizations (Allison, 1971; Steinbruner, 1974; Snyder and Diesing, 1977)" (1984, 114).

19. Allison mentions this advantage once (1971, 80). He does not, however, emphasize it.

20. "If the SOPs are appropriate, average performance—i.e., performance averaged over the range of cases—is better than it would be if each instance were approached individually (given fixed talent, timing, and resource constraints)" (Allison 1971, 89). He immediately adds, however, "But specific instances, particularly critical instances that typically do not have 'standard' characteristics, are often handled sluggishly or inappropriately" (ibid).

21. See also: "An important objective of standardization is to widen as far as possible the range of situations that can be handled by combination and recombination of a relatively small number of elementary programs" (March and Simon 1958, 150).

22. In a striking essay, Norton Long also differed sharply with the viewpoint of *Essence of Decision*, chapter 4, arguing that it is the *bureaucracy* that via its "fact-finding [and] policy proposal . . . procedures," must discipline the tendency of *politicians* to manipulate "a free-wheeling world of rhetoric and emotion, . . . a world in which solid facts evaporate" (1954, 28). It is interesting to note that though Allison cited this essay in respect to Model III (1971, 315, n. 65), neither Model II nor Model III was influenced by its main theme.

23. Surprisingly, neither chapter 3 nor chapter 4 specifies an explicit baseline of evaluation: using routines and standard operating procedures constrains organizational behavior *compared to what*? To an organization that though still staffed by boundedly rational actors, did not use standard operating procedures? Or to how an organization with perfectly rational members would behave? Or to how a perfectly rational *individual* would behave? Since the answer is unclear, the reader is left to infer the baseline of comparison.

24. We do not underestimate the difficulty of determining whether actors' disagreements are due primarily to conflicting goals or to conflicting beliefs. In a debate involving a president, insiders whose goals differ from the president's may find it wise to pretend their disagreements are about means. As March and Simon observe about organizations, "Bargaining (when it occurs) will frequently be concealed within an analytic framework" (1958, 131).

25. After criticizing what he considers Allison's overemphasis on bargaining in foreign policy formation, Rourke does remark that "in domestic policy, on the other hand, models structured around bargaining are much more useful, since executive organizations here compete with the President on a relatively equal footing" (1972, 432).

26. While Allison cites Dahl and Lindblom in support of his point that Huntington overemphasized participant equality (1971, 315, n. 65), it is interesting to note that Dahl and Lindblom consistently maintain that outside political support enables subordinates to bargain with the president (1953, 341–44).

27. In the missile crisis, for example, President Kennedy was apparently uncertain whether an air strike would destroy *all* the Soviet missiles in Cuba. When he sought advice from the Tactical Air Command, its commander told him that no such guarantee was possible. This judgment apparently played a major role in Kennedy's ruling out the air strike option.

28. To be sure, there were leaks to the press from inside the executive branch *before* the president and his top advisors became convinced that Soviet missiles were indeed in Cuba (Allison 1971, 192). Once the ExCom was convened, however, Allison mentions nothing further about leaks intended to influence policymaking.

29. Our conclusions here are in line with those of most of Allison's critics; see, esp., Art 1973; Ball 1974; and Krasner 1972.

30. We are not suggesting that politics occurs only within formal channels; evidence on the importance of informal organization is too strong to ignore. We do think, however, that the informal relationships that do emerge will be efforts to overcome the impact of the formal structure. Thus, formal structure shapes informal structure.

31. Allison himself bemoans "the difficulty of formulating Model III propositions about outcomes" (1971, 173).

32. We are reminded of Lewis Carroll's (1893) parable about map-making: "That's another thing we've learned from your Nation," said Mein Herr, "map-making. But we've carried it much further than you. What do you consider the largest map that would be really useful?" "About six inches to the mile." "Only six inches!" exclaimed Mein Herr. "We very soon got to six yards to the mile. Then we tried a hundred yards to the mile. And then came the grandest idea of all! We actually made a map of the country, on the scale of a mile to the mile!" "Have you used it much?" I enquired. "It has never been spread out, yet," said Mein Herr: "The farmers objected: they said it would cover the whole country, and shut out the sunlight! So we now use the country itself, as its own map, and I assure you it does nearly as well."

33. For a more detailed discussion of these trade-offs and of the great heuristic value of conceptual frameworks (partially formalized models), see Bendor and Hammond 1989.

34. For evidence that Allison took the historian's job of explaining the missile crisis seriously, see 1971, 1–2. Here he asks four central questions. The first three—Why did the Soviet Union place strategic offensive missiles in Cuba? Why did the United States respond with a naval quarantine? Why were the missiles withdrawn?—concern particular aspects of this particular event. As such, they are indistinguishable from those posed by historians.

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ON THE NATURE OF SUPREME COURT DECISION MAKING

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How does the U.S. Supreme Court reach decisions? Since the 1940s, scholars have focused on two distinct explanations. The legal *model suggests that the rule of law (stare decisis) is the key determinant. The* extralegal *model posits that an array of sociological, psychological, and political factors produce judicial outcomes. To determine which model better accounted for judicial decisions, we used Supreme Court cases involving the imposition of the death penalty since 1972 and estimated and evaluated the models' success in accounting for decisional outcomes. Although both models performed quite satisfactorily, they possessed disturbing weaknesses. The legal perspective overpredicted liberal outcomes, the extralegal model conservative ones. Given these results, we tested another proposition, namely that extralegal and legal frameworks present codependent, not mutually exclusive, explanations of decision making. Based on these results, we offer an integrated model of Supreme Court decision making that contemplates a range of political and environmental forces and doctrinal constraints.*

> The common law's . . . reverence for . . . *stare decisis* [means] that a policy made in a piecemeal manner would be better able to win support. . . . When the Court reverses itself or makes new law out of whole cloth . . . the holy rite of judges consulting a higher law loses some of its mysterious power.
>
> —Walter F. Murphy

> Judges are inevitably participants in the process of public policy formation; . . . they do in fact "make law"; [and] in making law they are necessarily guided in part by their personal conceptions of justice and public policy.
>
> —C. Herman Pritchett

How does the U.S. Supreme Court reach decisions? The above statements answer that question quite differently. The first, grounded in positivist jurisprudence, suggests that the rule of law stare decisis is the key determinant. The second, reflecting political jurisprudence, posits the converse—that an array of sociological, psychological, and political factors having little to do with legal doctrine produce judicial outcomes. These perspectives represent the mainstream—albeit highly divergent—responses to the question we pose. That both have persisted, despite the obvious tension between them, is somewhat puzzling; that the subfield of law, courts, and judicial processes may be more polarized than ever is extraordinary. While Hensley and Rhoads (1988) reported that the political (extralegal) model dominates disciplinary research, Slotnick (1989) noted the opposite trend—that the studies conducted by entering professionals reflect a return to doctrinal (legal) analysis.

Can two highly divergent conceptualizations of judicial decision making coexist? Empirical evidence suggests that at minimum, each can account for variations in behavior. Pritchett and many other adherents of extra legalism have more than ably demonstrated the explanatory power of their approach (e.g., Pritchett 1948; Schubert 1954; Spaeth 1961, 1979; Ulmer 1960). Segal and others present equally compelling results supporting the legal model (e.g., Hagle 1989; Kort 1957; Segal 1984; Wolpert 1991).

Yet for all the research conducted and all the years this debate has raged, it is almost startling to find that scholars have yet to compare these basic models systematically.[1] Pritchett and the extralegalists virtually ignore doctrine; Segal and others consider some attitudinal variables, but only after controlling for doctrine.

How legal and extralegal models of judicial decision making perform in head-to-head competition forms our central concern here. Using Court cases involving the imposition of the death penalty since 1972, we estimate and evaluate the ability of each to account for decisional outcomes.

CONCEPTUALIZATIONS

In his classic study of the various schools of jurisprudential thought, Pound (1959) identified over 20. Today, we probably could add 4 or 5 more (see Carter 1985; Stumpf 1988). In fact, debates—some more heated than ever—over how judges and justices *should* contemplate cases continue to occupy a preeminent position in legal and political circles (see "Critical Legal Studies Symposium" 1984; Posner 1981). We, on the other hand, are concerned with how justices *do* reach decisions. On some level the answers to this would probably rival in number those to the more normative query. To be sure, many—if not most—of the various jurisprudential schools find some representation within the nation's vast judicial apparatus. Within the political science literature, however, only two have received serious theoretical and analytical attention: the legal and the extralegal models.

The Legal Model

Often termed *positivist*, or *analytical, jurisprudence* (see Austin 1904), the *legal model* was the primary modus operandi for political scientists through the 1940s and continues to dominate legal education in the United States. At its core, legalism centers around a rather simple assumption about judicial decision making, namely, that legal doctrine, generated by past cases, is the primary determinant of extant case outcomes. It views judges as constrained decision makers who "will base their opinions on precedent and will adhere to the doctrine of *stare decisis*" (Wasby 1988, 210). Some scholars label this *mechanical jurisprudence*, because the process by which judges reach decisions is highly structured. Levi (1949) describes the basic pattern of legal reasoning, reasoning by example, as a process characterized by the doctrine of precedent and articulated in three steps: (1) observation of a similarity between cases, (2) announcement of the rule of law inherent in the first case, and (3) application of that rule to the second case.

Legal education and scholarship adopted this as a paradigm—the process by which judges and lawyers should proceed. Eschewing normative approaches, political scientists (from the 1920s through the 1950s) instead viewed "reasoning by example" as the way judges *do* proceed. Cushman (1929), Corwin (1924), and many others centered their work on the notion that previously announced legal doctrine provided the single best predictor and explainer of Court decisions.

How this model became so entrenched in the political science literature is readily discernible. Many scholars reasoned that judges (all schooled in legalism) would naturally gravitate to it upon ascension to the bench. After all, how else would they approach decision making? In addition, strong expectations exist within the legal community that judges will reason by example. As Murphy (1964) suggests, following legal precedents is a well-entrenched norm of the deliberative process. Finally, the case studies of the day reinforced the model's value. Articles published in political science journals summarized the reasoning used and the precedents set by the justices, disregarding any other factors contributing to decisional outcomes. Cushman's analysis of constitutional law for 1936–37 (one of the most volatile terms in the Court's history), provides a wonderful example. After acknowledging "The 1936 term of the Supreme Court will probably be rated as notable," he enumerated some of the "facts . . . one should bear in mind"—that Roosevelt had won a landslide reelection and had submitted a plan to reorganize the Court. Rather than demonstrate how those "facts" might have affected Court decisions, however, Cushman noted simply that "no suggestion is made as to what inferences, if any, might be drawn from them" (1938, 278) and proceeded to analyze the New Deal cases vis-à-vis existing precedent (a difficult task, indeed).

The Extralegal Model

While the legal model was predominating political science thinking about the Court, new perspectives emerged from the ranks of the nation's judiciary and law schools. In general, these thinkers denounced legalism as mechanical jurisprudence—sometimes subtly (e.g., Holmes 1881), sometimes loudly (e.g., Pound 1931). Instead, they beckoned judges to consider more dynamic factors as bases for decisions. Many credit Holmes's (1881) *The Common Law* with initiating this plea. Students of this school often cite as exemplary his remark that "the life of the law has not been logic; it has been experience. . . . It cannot be dealt with as if it contained only the axioms and corollaries of a book of mathematics" (1–2). Illustrative, too, is Louis Brandeis's famous brief in *Muller* v. *Oregon* 1908, containing 113 pages of sociological data but only 2 of legal argument. It was Pound (1931), however, who catalyzed the first strain of extrajudicialism, sometimes called *sociological jurisprudence*. In his seminal *Harvard Law Review* article, Pound drew his now-famous distinction between "law in books" and "law in action," behooving judges to adopt the latter, without necessarily abandoning the former. Cardozo and many others followed suit.

Later adapters of sociological jurisprudence—the realists of the 1930s—though were far more radical in orientation, maintaining that rules based on precedents were nothing more than smokescreens (see Frank 1930, 1950; Llewellyn 1951) or "myths, clung to by man out of a childish need for sureness and security. A mature jurisprudence recognizes that there is no certainty in law" (Stumpf 1988, 16).

So began a long line of thinkers who harshly critiqued legal reasoning for its inadequacy as a basis for judicial decision making, an inadequacy stemming from various considerations. From a normative standpoint, many followed Brandeis's lead, arguing that extralegal factors should enter judicial deliberations. After all, if judges were constrained by precedent, law would remain static when it should reflect changing morals and values. Additionally, critics argued that justices, "like other human beings, are influenced by the values and attitudes learned in childhood" (Frank 1950). It would be extraordinary, they claimed, to think that judges, just because they don black robes, were any less susceptible to such influences. Indeed, justices may be even more vulnerable than other decision makers because the rules of law are "typically available to support either side" (Pritchett 1969, 31). In making choices between competing precedents, then, other factors are bound to come into play.

Although legal realism gained a strong following within the nation's leading law schools during the 1930s, political scientists were reluctant adherents. It was not until the publication of Pritchett's (1948) *Roosevelt Court* that students began to abandon a positivist approach and view Court decisions more critically and analytically. In essence, Pritchett brought legal realism to political science. He equipped scholars with the arsenal necessary to estimate and evaluate its propositions. And, as an important by-product, he provided the fodder for the development of political jurisprudence and behavioralism—a development more fully stylized and realized by Schubert (1954), Spaeth (1961), and Ulmer (1960), who incorporated the assumptions of realism into their models of judicial decision making. Like Frank, they viewed the Court's environment as one that provided the justices with "great freedom to base their decisions solely upon personal policy preferences" (Rohde and Spaeth 1976, 72). But unlike the realists, they proceeded operationally to define and systematically to test attitudinal models of judicial behavior.

MODELS: CURRENT STATUS

The Extralegal Model

The underlying premise of *The Roosevelt Court*—that a host of internal, attitudinal factors other than "the law" contribute to case outcomes—continues to constitute a mainstay of research on Supreme Court decision making. Over the past decades, countless efforts have viewed court decisions as a function of personal attributes (e.g., Tate 1981), attitudes (Rohde and Spaeth 1976), and role conceptions (Gibson 1978). In general, however, most of these depict a Court driven by single-minded seekers of legal policy, justices who wish to etch into law their personal views.

Though the basic tenets of behavioralism continue to shape our vision of the judicial process, we have made some attempts at refining that conceptualization. In general, we now know that Court decisions are a product of factors not only internal but also external to the Court. Research by myriad scholars reveals that litigants, attorneys, and parties can affect case outcomes (e.g., Galanter 1974; Vose 1959). Although this body of literature is, conceptually speaking, quite murky, we acknowledge the validity of several of its basic premises. First, we recognize that attainment of "repeat player status" can translate into litigation success (Galanter 1974). How one becomes a "repeat player" is difficult to specify; but at least two factors contribute: association with a national litigating organization and regular appearance before the Court. Attorneys from the ranks of public interest law firms are ensured some level of financial and other support that many private counsel lack (Vose 1959). Repeated and continuous use of legal recourse breeds familiarity, confidence, and expertise that one-shotters cannot possibly acquire (O'Connor 1980). Second, and concomitantly, we acknowledge the extraordinary role played by one repeat player, the solicitor general of the United States. Whether participating as a direct party or as an amicus curiae, solicitors general seem to carry great weight with the justices. Their presumed legal expertise, their utility in preconference screening, and their embodiment of the interests of the nation are just some of the factors explaining the solicitors' general excellent record before the Court (Segal 1991). Third, we accept the proposition that appellants are more likely to win their cases than appellees. This expectation is grounded both in fact (Wasby 1988) and in theory (Palmer 1982). Thus, no extralegal model can afford to ignore it.

Political scientists also recognize that pressures emanating from outside the legal system can affect case outcomes. In particular, we believe that the Court is responsive to the larger political environment. Some suggest that its rulings reflect the wishes of the citizenry (e.g., Barnum 1985). But owing to difficult conceptual and analytical obstacles, scholars have yet to provide truly compelling support for this assertion (see Caldeira 1991, 313–16). They have been more successful at establishing a link between the Court and the other branches of government. That is, even though the Justices lack an electoral connection or mandate of responsiveness, we have several reasons to suspect that Congress and the president affect Supreme Court decision making (Casper 1972; Murphy 1964) and that the direction of their influence reflects their partisan composition (Nagel 1969). First, scholars assert that both Congress and the president possess a vast array of powers over matters important to the Court, which the Justices can hardly afford to ignore (Baum 1989a, 133). Some of these powers, such as presidential and senatorial roles in the appointment process, are obvious; some are not. As Wasby (1988) notes, Congress can restrict the Court's jurisdiction to hear cases; enact legislation, or even propose constitutional amendments, to recast Court decisions; and hold judicial salaries constant. In turn, "the Court's policies may be affected by a desire to deter Congress" from taking these steps (Baum 1989a, 133). By the same token, Murphy (1964) and Scigliano (1971) point to the multifaceted relationship between the Court and the president, one that goes well beyond his power to nominate justices or even to send his solicitor general into the Marble Palace. It is no secret, for example, that many presidents have enjoyed special relationships with sitting justices and that presidential preferences have found their way into particular opinions. Nor can we ignore the role the president plays in shaping congressional responses to the Court.

Another set of literature focuses on the role of the Court within a democratic society, many scholars arguing that Court decisions do not significantly deviate from the preferences of the existing majoritarian regime. Though some argue that this is largely a result of the regularity with which presidents appoint justices (e.g., Dahl 1957), others suggest that it stems, at least in part, from the institutional nature of the Court (Marshall 1989; Murphy 1964). In short, because the justices lack any real mechanism to enforce their decisions, they depend on other political actors (and the public) for support of their positions (Johnson and Canon 1984). Without such support, the Court could lose its institutional legitimacy, a danger of which its members seem well aware (Caldeira 1991). Accordingly, for the Court to have power within government it must "follow the election returns," or at the very least, consider them (Barnum 1985). Along similar lines, the Court, at least on occasion, takes cues from the political environment to guide its decisions (Vidmar and Ellsworth 1974). It has even gone so far as to build that dimension into some of its jurisprudential standards. In evaluating whether certain kinds of punishments violate the Eighth Amendment's cruel-and-unusual clause, for example, the Court proclaimed that it would look toward "evolving standards of decency" as defined by public morals.

Given this literature, then, we have sufficient reasons to suspect that the president and Congress can affect Supreme Court decision making or at least that Court rulings encapsulate the preferences of the other branches. What is more, we know something about the direction of this influence. The relationship between the Court and other branches of government, throughout history, has revolved around the ebb and flow of partisan politics. As Nagel observed, these patterns are "significantly shaped by such political considerations as the degree of party difference between Congress and the Court, the nature of the party in power in the national government and the party affiliations" of the individual Justices. (1969, 259). Put in more concrete terms, when the nation and its institutions move toward parties representing "have not" views (e.g., today's Democratic party), the Court tends to adopt more liberal policy postures, particularly on issues of rights and liberties. Conversely, as parties of "haves" (e.g., today's Republican party) come into power, the opposite effect will occur (Epstein, Walker, and Dixon 1989; Nagel 1969). In sum, while we continue to incorporate the premises of the attitudinal approach into our thinking about behavior, we also recognize that pressures external to the Court (be they from attorneys, parties, and litigants or from the larger political environment) supplement behavioral assumptions. We can express this extralegal model as

$$\text{Pr}\ (C_i = 1) = b_0 + \Sigma b_h \text{EL}_{ih} + e, \qquad (1)$$

where C_i is the Court's decision in a given case (a binary variable equal to 1 if the Court rules in a conservative direction, 0 otherwise); $\text{Pr}(C_i = 1)$ is the probability of a given Court decision taking a conservative posture; and EL_{ih} are the extralegal variables influencing that decision.

The Legal Model

It is, of course, true that many of those engaged in the study of public law are less than content with this extralegal model, calling for a greater emphasis on doctrine as a valid explanation of case outcomes. Brigham (1978), Johnson (1987), and O'Neill (1981) have stressed that the language of law (precedents, etc.) channels and constrains judicial choices. Shapiro (1972), too, suggests that "we have overemphasized extralegalism to the point [that] we somehow ignore the fact that appellate courts and the lawyers that serve them spend an overwhelming proportion of their energies communicating with one another, and that the judicial opinion itself conforming to the style of *stare decisis*, is the principal mode of communication." Two decades after publication of *The Roosevelt Court*, Pritchett even lamented that behavioralism may have gone too far: "Political scientists who have done so much to put the 'political' in 'political jurisprudence' need to emphasize that it is still 'jurisprudence'. It is judging in a political context, but it is still judging; and, judging is something different from legislating or administering" (1969, 42).

Like behavioral models, legal perspectives have evolved over the years, not so much conceptually as analytically.[2] In particular, work by Kort (1957) and Segal (1984, 1986) demonstrated systematically—not contextually—that the law controls case outcomes. Rather than focus on a specific decision or strain of precedent, Kort and Segal isolated areas of law and used facts and legal doctrine, respectively, to predict judicial determinations. When they found that the Court responded to legal factors in a consistent manner, some social scientists began to question the sweeping generalizations of the behavioralist school.

Kort's (1957) work has been the subject of some criticism. Fisher (1958) chastised his methods; others found his use of legally irrelevant facts puzzling.[3] Segal's analyses overcame some of these shortcomings. He used probit (a multivariate analytic strategy), which allowed him to reach more precise conclusions about the relationship between doctrine and output. In addition, he limited his consideration to facts of legal significance. Virtually all his variables tap doctrine, rather than attributes of the sort incorporated by Kort.[4] Indeed, he labels his analysis "a legal model."

Even so, Segal's model (and similar ones, e.g., Hagle 1989; Ignagni 1990; Wolpert 1991) arguably failed to correct for some of the shortcomings of previous work. Scholars continue to construct legal models in a rather post hoc fashion. Segal, for example, included certain variables (e.g., warrantless searches made prior to arrest and consent searches) because the *Court* said they were important and not because he had a priori knowledge that they should be significant in search-and-seizure analysis. Another potential problem is more temporal in nature: most legal models incorporate doctrine that the Court enunciated *after* it decided some of the cases contemplated in a given analysis. Segal included a variable comprised of six "exceptions" to the warrant and probable cause requirements. Although the Court had not written any of these exceptions into law prior to 1967 (the balance came in the early 1970s), Segal incorporated them into a model developed to explain

Court search-and-seizure decisions between 1962 and 1981.

Do these problems undermine the efforts of Segal and those following in his footsteps? We think not. On the contrary, they are endemic to legal models in general, rather than to specific scholarly efforts. Consider the temporal problem from the perspective of those who view the law as self-contained (as it is often presented to be in law school courses). Because, for them, the core notion is that all decisions are rooted in stare decisis, they would simply respond that the old doctrine anticipated the newer one, that the facts and questions varied, not the doctrine. They would bring much the same argument to bear on the post hoc nature of these models.

Seen in this light, the approach taken by Segal and others (Hagle 1989; Ignagni 1990; Wolpert 1991) neatly parallels the conceptualization offered by legalists. What is more, by providing empirical verification for the legal model, they gave it greater status within the scientific community studying judicial decision making.[5] Though their methods differed from those of the classicists and they often included attitudinal measures in their models, their conclusions lent support to traditional assumptions about the nature of legal deliberations. More formally, we could conceptualize this model as

$$\Pr(C_i = 1) = b_0 + \Sigma b_j L_{ij} + e, \qquad (2)$$

where L_{ij} are the legal factors impinging on the probability of a conservative decision and the other symbols are as in equation 1.

ESTIMATING THE MODELS

Since both legal and extralegal models are well established theoretically and analytically, we evaluate each on two scores: the typical parameter estimation strategy[6] and a strategy based on the ability of the models to classify Court decisions correctly. For this examination, we focus on almost two decades' worth of cases involving capital punishment ($N = 64$), measuring Court outcomes as a simple binary choice whether it affirmed the imposition of the penalty (coded 1) or not (coded 0). Data are from the *U.S. Reports* for the 1971–88 terms. The mean value across terms is .45 in favor of the death penalty.

In general, our task in operationalizing the independent variables for both the legal and extralegal models is rather straightforward. As we shall describe in some detail, we simply adapt past constructs to our data, rather than improve upon them or introduce *major* innovations. Given our interest in comparing two well-entrenched (and empirically verified) models, we believe this to constitute the most evenhanded approach. Nonetheless, as our discussion has indicated, we are not unaware of the shortfalls of past research exploring the relative effect of extralegal factors and legal doctrine on decision making. There is still debate, for example, over whether factors exogenous to cases actually affect the Court's decisions. By the same token, while Segal's (1984) work markedly advanced the line of inquiry on the impact of doctrine, that does not necessarily mean that it and its progeny are perfect. Nor can we ignore the perspective that *any given* Court producing *any given* legal doctrine is composed of justices possessing particular attitudes and so forth. Thus, a certain degree of interaction undoubtedly exists.

We shall take up some of these concerns later. Our present task is to examine the relative performance of the models as models. In so doing, and for the reason suggested, we feel it important to follow a research tack similar to those which preceded us in terms of the development of the model. With this in mind, we now turn to our specific operationalizations of both models.

The Legal Model

To develop a legal model, we followed Segal's lead. In his research on search-and-seizure cases, he reviewed Court doctrine relating to these suits and specified a concomitant legal model around that doctrine. This strikes us as a sensible approach, one that can clearly delineate whether "the law" determines case resolution. In general, the Court has centered its inquiry into capital punishment on the decision-making process, that is, the decision to impose death or not.[7] More specifically, it has promulgated legal doctrine in response to three orienting questions. First, upon whom can decision makers confer a sentence of death? Rape[8] and, in fact, all crimes for which the defendant did not intend to murder are undeserving of capital punishment. Hence, if the accused was tried for anything less than intentional murder,[9] we would expect to find the Court resolving the case in favor of the criminally accused. To test this proposition, we coded the alleged crime (CR) as one for which a sentence of capital punishment was proportional to the offense (coded 1) or not (coded 0).

Second, who can make the decision between life and death? Although the Court allows judges and/or juries to impose a sentence of death,[10] it has made clear that the latter cannot be biased toward such a verdict. Juries, in short, cannot be *death-qualified* (DQ)[11] that is, the "death penalty may not be imposed if the jury that assessed it was selected so as to exclude anyone who expressed general objections to capital punishment or religious or conscientious scruples against it" (Whitebread and Slobogin 1986, 637). To determine whether the justices responded consistently to issues of death-qualified juries, we coded whether this claim was raised (coded 0) or not (coded 1). If it was part of the legal record, we expect the Court to find in favor of the defendant.

Finally, what factors should decision makers consider in their deliberations? Clearly, judges and juries must, in an evenhanded manner, consider whether a penalty of death is the appropriate one. For instance, they must look at the particularized circumstances

(PC) of the case that may favor the defendant's position.[12] Along similar lines, the death penalty cannot be made mandatory,[13] nor, conversely, can it be applied in an arbitrary (e.g., racially discriminatory) manner.[14] Certainly, then, we would expect the Court to look unfavorably on any attempt to limit factors idiosyncratic to the crime or the accused (coded 0 if present, 1 if not). Concomitantly, judges and juries should contemplate particularized aspects of the crime against those factors in aggravation (AG). Unlike factors mitigating against death, those in aggravation must be specific and appropriate.[15] If they are overly broad or vague, the Court should be inclined to rule against the state. We coded the presence of such a claim as 0, the absence as 1. In addition, juries and judges often consider, at the prosecution's urging, results of state-initiated psychiatric (SP) examinations. The Court, however, has clearly indicated that these must be conducted in accord with strict norms of due process and fairness.[16] We posit, then, that a claim suggesting otherwise (coded 0) would weigh in favor of the accused.

Given these definitions, we can now rewrite equation 2 in the following operational terms:

$$\Pr(C_i = 1) = b_0 + b_1 CR_i + b_2 DQ_i + b_3 PC_i + b_4 AG_i + b_5 SP_i + e.$$

We expect $b_1, b_2, b_3, b_4, b_5 > 0$. Like Segal, we recognize that some death penalty cases "often have idiosyncratic factors that affect the Court's decision and [that these] factors do not determine the Court's resolution of any case" (1984, 893). Yet we also argue that the factors considered here should strongly predispose the Court toward finding against the imposition of the death penalty.

This noted, one issue remains—the data collection process. Because it is likely that Court opinions filter out specific claims, they are not (for our purposes) reliable or useful sources of information. Accordingly, scholars seeking to estimate legal models turn to outside sources, such as the briefs filed by participating attorneys or the decisions of lower courts. Because attorneys narrow the array of existing legal arguments from which the justices can select to focus their opinions, we used their claims as our primary data source. For some of the doctrinal variables encompassed in our legal model (e.g., death-qualified juries), however, we found it necessary to consult the lower court opinions to ensure that they were, in fact, part of the legal record.

The ExtraLegal Model

Given previous successes at estimating extralegal models, our task is rather straightforward. As we did for the legal model, we simply adapt past constructs to our data. We earlier demonstrated that three elements compose this perspective: the justices' internal predispositions, litigants, and the political environment. Although previous research accounts for internal value changes in diverse ways, one acceptable procedure operates under the assumption that as the number of Nixon, Ford, and Reagan appointees increased, so should the number of conservative (in our case, pro–death penalty) outcomes (e.g., Segal 1984). We can, then, operationalize this Court change (CC) variable as an additive index, taking on the value of 0 until the appointment of Stevens, 1 after Stevens, 2 with the appointment of O'Connor, 3 after Scalia, and 4 with Kennedy.[17]

Measuring the literature's expectations about the role of parties in litigation means adding of four factors. First, we consider whether attorneys representing defendants were experts in death penalty litigation. Following the lead of others, we assume that defense counsel (DC) from the ranks of public interest law firms or interest groups (in our study, virtually all were from the ACLU or the NAACP Legal Defense Fund) and those appointed by the Court were experienced death penalty litigators (coded 0), otherwise not (coded 1). Second, to avoid underestimating our model, we considered the relative expertise and repeat player status of the states (ST). We reasoned that the more a state litigated in this particular area, the greater its expertise (see, generally, Epstein and O'Connor, 1988). Three of the 18 states involved in death penalty litigation—Texas, Georgia, and Florida—accounted for nearly 50% of all cases, each litigating roughly 10 of them. We coded them as repeat players (1), all others as 0. Third, we ascertained whether the solicitor general (SG) had filed a brief amicus curiae (coded 1) or not (coded 0);[18] and we considered who appealed (AP) to the Court, the defendant (coded 0) or the state (coded 1).

Finally, we wanted to capture appropriately the political environment confronting the Court. Not only is this a factor that all extralegal models should consider, but, because the Court has incorporated an "evolving standards of decency" component into its capital punishment jurisprudence, it is of particular importance for our analysis. The logic proffered by other scholars, pointing to the obstacles of relying on public opinion data and of the benefits of considering the composition of the other national institutions, led us to adapt an index to tap political pressures (from Cameron, Cover, and Segal 1989). Here, we assume that as the political environment (PE) becomes more Republican, so will the Court's decisions. That environment takes on a value of 0 if both the president and Congress are Democratic; a value of 1 if one branch is Republican, the other Democratic; and a value of 2 when the Senate and the president are Republican.

In sum, we can now specify the extralegal model, as depicted in equation 1, in the following terms:

$$\Pr(C_i = 1) = b_0 + b_1 CC_i + b_2 DC_i + b_3 ST_i + b_4 SG_i + b_5 AP_i + b_6 PE_i + e.$$

We expect $b_1, b_2, b_3, b_4, b_5, b_6 > 0$.

EVALUATING THE MODELS: PARAMETER ESTIMATES

Table 1 depicts the estimates retrieved from a probit analysis of the legal and extralegal models.[19] As the maximum likelihood estimates and summary measures indicate, both perform quite satisfactorily. The −2 × log-likelihood ratios lead us to reject the implicit null hypothesis (i.e., all coefficients equal 0) for both. Moreover, they do an equally adequate job in reducing error prediction. If we had no knowledge of legal or extralegal factors affecting death penalty case outcomes and we guessed that all were liberal, we would accurately classify 55%. The legal model, which correctly categorizes 75% of the cases, results in a 44% reduction in error,[20] and the extralegal model, which accurately classifies 81%, reduces error by 58%.[21]

Although our concern lies with the models as models, the individual coefficients of each are rather interesting. In general, the legal model's maximum likelihood estimates meet our expectation. The absence of supportive doctrine positively affects the Court's propensity to take a law-and-order stance. More specifically, we hypothesized that the justices centered their inquiry into capital punishment on three dimensions of the decision-making process. We suggested, for example, that they would be less likely to uphold a death sentence against a defendant who did not intend for murder to occur. As we can see, when such a sentence is imposed, it results in a 1.49-standard-deviation addition to the cumulative normal probability function. More concretely, a death sentence imposed on an intentional murder would have a .51 probability of Court affirmation, one given to any other defendant a .12 probability.

We also suggested that several doctrinal aspects of the jury–judge deliberation process would affect Court disposition: the specifics of the crime, aggravating circumstances, and state-initiated psychiatric inquiries. As noted, all three have some impact on the justices. Failure to consider particularized case circumstances is one of the more important determinants, actually altering institutional choice. If defendants raise this issue, the model predicts an outcome favoring them; if not, the Court will probably find for the state. Charges of overly broad or inappropriate aggravating circumstances also increase substantially the probability of the Court's reaching a liberal outcome. The results of a modal categorization indicate that the state will likely lose if this issue is raised (.47) but will win in the absence of such a claim (.82). So, too, the Court has scrutinized quite closely state-initiated psychiatric proceedings. Sentences of death involving such issues would have only a .28 probability of being upheld as compared to .48 without them.

The third issue we expected the Court to consider—death-qualified juries—failed to produce a significant coefficient. Its estimate, however, requires little explication. The variable adds to the model's explanatory power,[22] and its maximum likelihood estimate is positively correlated with a prodefendant outcome, conforming to our hypothetical model. Further, compare the modal probabilities for sentences reached by death-qualified juries (.50) versus those of all others (.82).

Table 1

Probit Estimates for Legal and Extralegal Models of Decision Making Capital Punishment Cases, 1971–1988 Terms of the U.S. Supreme Court

VARIABLE	LEGAL MODEL COEFFICIENTS	EXTRALEGAL MODEL COEFFICIENTS
Death-Qualified (DQ)	.94* (.61)	—
Crime (CR)	1.49** (.73)	—
Particularized circumstances (PC)	1.46*** (.44)	—
Aggravating factors (AG)	1.01** (.50)	—
State psychiatric examination (SP)	1.38** (.62)	—
Political environment (PE)	—	1.88*** (.60)
Court change (CC)	—	.70** (.30)
Appellant (AP)	—	1.74*** (.57)
Defendant counsel (DC)	—	1.20** (.56)
State (ST)	—	1.29*** (.49)
Solicitor general (SG)	—	2.67*** (1.07)
Constant	−5.35	−5.87
−2 × log-likelihood ratio	18.82***	36.69***
% categorized correctly	75	81

Note: Standard errors are in parentheses. N = 64. Of these cases, the Court affirmed the imposition of the death penalty in 45%.
*$p \le .10$.
**$p \le .05$.
***$p \le .01$.

The maximum likelihood estimates of the extralegal model are also satisfactory, with our measures of internal and external factors producing significant results. Consider, first, the internal change variables. We see that the addition of Republican appointees enhanced the Court's propensity toward a law-and-

Table 2

Evaluating the Predictions of Legal and Extralegal Models Over Time

NATURAL COURT ERA	NUMBER OF CASES PREDICTED CORRECTLY: LEGAL MODEL	NUMBER OF CASES PREDICTED CORRECTLY: EXTRALEGAL MODEL	TOTAL NUMBER OF CASES
1 (1971–74)	3 (100)	3 (100)	3
2 (1975–80)	16 (94)	17 (100)	17
3 (1981–86)	19 (63)	22 (73)	30
4 (1987–88)	10 (71)	10 (71)	14
Total	48 (75)	52 (81)	64

Note: Prediction equations are probits using estimates displayed in Table 1. Percentages are in parentheses.

order stance. A most significant shift, however, occurred when O'Connor joined the Court. Prior to her ascension to the bench, a death sentence probably would not have been upheld (a .20 probability). Upon her arrival in 1981, odds increased to .69 that the Court would hold for the state. The external political environment also exhibited a *strong* effect on Court behavior, the most obvious manifestation occurring in the Carter-to-Reagan transition.[23] During the Carter administration, when the government was uniformly Democratic, sentences of death had only a .37 probability of Court approval. During the first six years of the Reagan presidency, the Court almost always upheld such verdicts (.69). Two of the litigant variables also fared quite well. If the solicitor general filed a brief supporting the state, the odds approach .61 of the Court's concurring. In the absence of solicitor general reinforcement, states will generally lose their cases (.44 probability). Likewise, when the defendant appealed, the Court would probably take a liberal position (.65). If the state appealed, that probability would fall to .24. Hence, once again, the solicitor general and appellant variables produced a radical change in institutional decision making from a stance favoring defendants to one predisposed toward state interests.

Though the attorney and state variables did not have quite as dramatic an effect on average, they performed adequately. States facing a less-than-experienced attorney would have about a 50/50 chance of winning their dispute. But if they opposed interest group counsel or a Court-appointed defender, their odds fell, slightly, to 42%. Conversely, attorneys challenging practices in Texas, Georgia, or Florida faced an uphill battle as the modal probability approached .75 that the Court would find in favor of those states; it neared 0 for other governmental litigators.

Both the overall fits and the estimates of the extralegal and legal models suggest that the Court reacted quite consistently to doctrinal cues *as well as* to changes in political stimuli presented in over 15 years of death penalty litigation.

EVALUATING THE MODELS: CATEGORIZATIONS AND PREDICTIONS

Model evaluation can occur on a number of different dimensions. We already have detailed our models' ability to generate reasonable parameter estimates. Indeed, having found both to generate satisfactory coefficients, we could simply conclude that legal and extralegal explanations provide equally viable evaluations of Court decision making. This sort of analysis, however, reveals little about their relative value and their ability to postdict the range of case outcomes. Moreover, significant maximum likelihood estimates may mask individual, nonrandomized flaws. Because time-boundness has constituted such a problem in previous models of Court decision making (e.g., Ulmer 1986),[24] we are particularly interested to know whether the explanatory power of legal and/or extralegal perspectives diminishes over time. We address these concerns by comparing the percentages of correct and incorrect categorizations generated by both models over time. To facilitate interpretation, we grouped these into four periods roughly corresponding to the Natural Court eras included in our analysis.[25]

As we depict in Table 2, we find little longitudinal erosion, generally speaking. Both models start off quite well, postdicting nearly 100% of the 20 cases decided during the first two Natural Courts. Equally satisfying is that both categorize correctly 10 of the 14 cases occurring in Natural Court 4. Perhaps most important, however, is that their ability to generate correct predictions for the latest round of death penalty cases was nearly as good as those for the first. Our models correctly categorized all three cases decided in 1972. During the 1988 term, the Court decided 7 cases involving imposition of the death penalty. If we withhold those cases and reestimate the models, each accurately forecasts the outcomes in 5 of the 7.

Although no erosion occurred in the utility of either model, a more detailed examination reveals the

Table 3

Evaluating the Predictions of Legal and Extralegal Models by Ideology

	% PREDICTED						
	LIBERAL (AGAINST DEATH PENALTY)			CONSERVATIVE (FOR DEATH PENALTY)			
NATURAL COURT ERA	LEGAL	EXTRALEGAL	ACTUAL % LIBERAL	LEGAL	EXTRALEGAL	ACTUAL % CONSERVATIVE	TOTAL NUMBER OF CASES
1 (1971–74)	100	100	100	0	0	0	3
2 (1975–86)	76	82	82	24	18	18	17
3 (1981–86)	66	43	36	33	56	63	30
4 (1987–88)	79	21	50	21	79	50	14

Note: Prediction equations are probits using estimates displayed in Table 1.

presence of two nontrivial problems. First, as we can see in Table 3, which depicts the percentage of liberal and conservative outcomes produced during each natural court (in contrast to the actual outcomes), our models possess ideological biases. The political model becomes increasingly, monotonically, and *incorrectly* prone toward conservatism. For Natural Court 1, it correctly classified all outcomes as liberal; by Natural Court 4, it placed only 21% of cases into that category, when half actually belonged there. Thus, despite initial appearances, by Natural Court 4 the political model is overestimating conservatism, an important finding, to which we will return.

The legal model exhibits precisely the opposite propensity. Beginning in Natural Court 3, the legal model overestimates the *liberal* inclination of the Court. As Table 2 indicates, it yielded its worst predictions in Natural Court 3. As we can see in Table 3, it produced those errors because it overcategorized liberal outcomes. Natural Court 4 presents much the same picture. It classified all seven cases decided in favor of the defendant accurately, but only three of the seven outcomes supporting the state. In short, our doctrinal model predicts more prodefendant outcomes over time than actually occurred, while the extralegal one is moving in the opposite direction. This reaches a point of virtual juxtaposition in Natural Court 4, when the legal model suggested that 79% of the cases would fall into the liberal column, while its extralegal counterpart placed 79% into the conservative one.

This trended behavior continues to plague the models into the Court's 1989 term, which we did not include in our analysis, but can now consider for diagnostic purposes. During that term, the Court decided 11 death penalty cases, holding for the state in 10. If we did not know the outcomes of those cases and used the estimates generated from our legal and extralegal models (see Table 1) to predict them, our results would be discouraging. Both continued along the ideological paths already described. The legal model predicted that the Court would decide 10 of the 11 1989 term cases in favor of the defendant, and the extralegal model classified all 11 as holding for the state.[26]

Not only are the models exhibiting trended ideological behavior, but (a related problem) they also seem to be working at cross purposes. Consider that the legal model incorrectly classified 16 cases, the extralegal one 13. If the cases missed by the models were simply idiopathic, we would expect many of the total 29 to be shared errors; but in fact, they had only 4 missed cases in common. Put in somewhat different terms, of the 16 outcomes incorrectly classified by the legal model, the extralegal one picked up 12. Conversely, the legal model accurately categorized 9 of the 13 errors produced by the extralegal one.

Hence, what emerges (taken in conjunction with previous findings) is that while the overall fits are quite good, the models exhibit disturbing propensities and, interestingly, ones that move in precisely opposing directions. The political model becomes increasingly conservative, the legal model increasingly liberal. Thus, the errors they produce are largely idiosyncratic.

Explaining the Findings

Taken together, then, the legal and extralegal models seem to be working at cross purposes, producing opposing categorizations over time. Theoretically speaking, this is a puzzling phenomenon but one on which we can speculate. We first consider a traditional criticism of the legal model, namely, that it is too static to account for changes in the law. We found, however, that this was not necessarily the case for death penalty litigation. Rather, it does change over time, forecasting more liberal outcomes with each passing Natural Court era than actually happen.

Why this occurs is rather evident. Once the Court enunciates legal policies, attorneys will take a cue from those precedents, bringing better and better arguments within those doctrinal parameters. Quite exemplary are capital cases involving overly broad aggravating circumstances. In *Gregg* v. *Georgia* 1976, the defendant's attorney claimed that one of the state's aggravating circumstances, namely, that the offense was "outrageously or wantonly vile, horrible or inhuman in that it involved torture, depravity, or an aggravated battery to the victim," was overly broad and vague. The Court rejected this contention on the grounds that future courts need not construe it as such. This holding voided Gregg's particular claim but opened the doors to new challenges based on future interpretation of that circumstance. This is precisely what happened in *Godfrey* v. *Georgia* 1980, when an attorney challenged Georgia's construction of the very same aggravating circumstance. Based on *Gregg*, the justices agreed that the state courts had interpreted it in such a way as to make it overly broad and vague. Our legal model accurately classified the outcome of *Godfrey*; but it missed a 1989 case also involving aggravating circumstances, *Hildwin* v. *Florida*.[27] In this instance, the Court ignored existing precedent and found for the state.

After this small deviation from *Gregg* and *Godfrey* in 1989, the justices moved more rapidly to distinguish existing precedent. For example, in the 1990 case of *Lewis* v. *Jeffers*, they held that the aggravating factor challenged in *Gregg* was not unconstitutionally vague as applied to the defendant even though a lower court, relying in part on *Godfrey*, held that it was. Once again, we used existing estimates (see Table 1) to try to "postdict" *Lewis*. Not surprisingly, this effort failed. The legal model—built around earlier precedent—failed to categorize correctly the outcome of *Lewis* v. *Jeffers*.

This series of cases (and others) demonstrates that the legal model contains an inherent flaw. Because it only considers legally relevant facts, it will continue to forecast liberal outcomes as attorneys capitalize on existing precedent even though "the law" may not actually move in that direction. This is why our legal model accurately forecasted the Court's early "aggravating" cases but missed those decided in 1989 and 1990.

Does this mean that we should discount the legal model in favor of the extralegal one? We think not, because it, too, exhibits idiopathic behavior, prematurely predicting conservative outcomes. It accurately predicted *Hildwin* but missed several other cases in which the Court adhered to precedent and ruled in favor of the defendant.[28] Particularly interesting is the one 1989 term ruling that it miscategorized, *McKoy* v. *North Carolina*. This case involved the constitutionality of a state sentencing scheme permitting juries to consider only those mitigating circumstances on which they unanimously agreed. Because so many pro-state "buttons" of our existing extralegal model were pushed (e.g., the defendant was not represented by a group, the high number of Nixon–Reagan appointees, etc.), it predicted that this case had virtually no chance of being decided in favor of the defendant. Yet, citing clear-cut doctrine suggesting the need for "flexibility" of mitigating circumstances, the Court held for the defendant.

Seen in this light, the extralegal model is too dynamic. It assumes that changes in the political environment will contemporaneously affect court decisions. Yet as the death penalty data suggest, this may be an incorrect assumption. Why does the model possess this flaw? Most importantly, as *McKoy* indicates, it fails to consider the doctrinal boundaries in which the Court works. If Brigham (1978) and others are right, stare decisis does constrain the array of available legal options. Accordingly, abrupt alterations in the political environment may not necessarily translate into concomitant, contemporaneous doctrinal change.[29]

AN INTEGRATED MODEL

Hence, both models possess weaknesses—weaknesses that run diametrically to one another. The legal perspective, which overpredicts liberal outcomes, needs to be tempered by a consideration of political forces. Conversely, the extralegal model should be checked by doctrinal constraints. Given these weaknesses, should we abandon both models as incomplete portrayals of Court decision making? We think not. Rather, they should be considered codependent explanations. The legal model assumes that the Court cannot ignore its role as an adjudicative body, that it is bound by precedent. The extralegal model assumes that the Court heeds political forces and must do so to retain its institutional legitimacy. Over time, those models may diverge, applying diametric pressure of the Court. For capital punishment cases, the legal model exerts a liberalizing influence on the justices as attorneys capitalize on existing doctrine. The extralegal exerts influence toward a more conservative posture as the external and political environments move in that direction. To this extent, legal and extralegal models present interdependent explanations of judicial decision making.

Seen in this light, then, our most prudent approach, analytically speaking, is to combine the two into an integrated model of decision making, one informed by both legal and extralegal factors:

$$\Pr\,(C_i = 1) = b_0 + \Sigma b_h EL_{ih} + \Sigma b_j L_{ij} + e. \qquad (3)$$

Table 4 depicts the maximum likelihood estimates and summary statistics retrieved from estimating equation (3) in probit. As we can see, the model is quite well behaved. It is significant at the .01 level; all variables add to the model's explanatory power; and the maximum likelihood estimates are in the hypothesized direction. Even more encouraging is its ability to categorize correctly 88% of all case outcomes, with no evident ideological bias.[30]

Based on these results, it would be tempting to conclude that scholars should consider both legal and extralegal variables in their models of Court decision making, that these factors have an equivalently strong impact on outcomes. But is that true? To explore this, we created instruments for each set of factors (legal and extralegal) and then used those to generate beta weights.[31] Table 5 illustrates the results of that analysis. Overall, extralegal factors exert nearly twice the influence (.35 versus .69) on a given Court decision. Particularly interesting, however, is

Table 4

An Integrated Model of U.S. Supreme Court Decision Making

VARIABLE	COEFFICIENT
Death-qualified (DQ)	1.23 (1.14)
Crime (CR)	2.86** (1.21)
Particularized circumstances (PC)	1.13** (.66)
Aggravating factors (AG)	1.05* (.68)
State psychiatric examination (SP)	2.19*** (.86)
Political environment (PE)	2.25*** (.79)
Court change (CC)	.64* (.39)
Appellant (AP)	1.55** (.79)
Defendant counsel (DC)	1.08** (.62)
State (ST)	1.90*** (.64)
Solicitor general (SG)	1.93* (1.44)
Constant	−13.66
−2 × log-likelihood ratio	48.99***
% categorized correctly	88

Note: Standard errors are in parentheses. N = 64. Of these cases, the Court affirmed the imposition of the death penalty in 45%.
*$p \leq .10$.
**$p \leq .05$.
***$p \leq .01$.

the emerging pattern of influence. At first, doctrinal influences had quite a strong impact on the Court; but after Natural Court 1, they decline as extralegal factors set in.

Table 5

The Relative Influence of Legal and Extralegal Variables on U.S. Supreme Court Decisions

NATURAL COURT ERA	BETA WEIGHTS	
	LEGAL MODEL	EXTRALEGAL MODEL
1 (1971–74)	.52	.00
2 (1975–80)	.35	.67
3 (1981–86)	.48	.72
4 (1987–88)	.56	.80

Note: See note 31 for the computation of the beta weights.

Given our models juxtaposed against evolving doctrine governing the imposition of capital punishment, the data depicted in Table 5 make a great deal of sense. When the Court handed down *Furman* v. *Georgia*, it was writing on a virtually clean slate. Interestingly, however, extralegal factors have no discernible impact on it. The variables included in our legal model drove the decision. To some extent, this is an artifact of how we derived that model—mainly, though not exclusively, from *Furman* (and *Gregg*). But given the relative weight of extralegal factors (nonexistent), the data do tell us something about the nature of judicial decision making. When the justices placed the relatively novel issue of capital punishment on their agenda in 1972, they were apparently more influenced by the extant legal claims than by political and societal forces, which had yet to gel sufficiently. Such a finding seems to support what others have written about those early death penalty cases (e.g., Kurland 1972).

What we see in Natural Courts 2, 3, and 4 also fits compatibly with our existing knowledge of capital punishment doctrine and of Court decision making. As the issue evolved and doctrine was refined (or, in some instances, severely distinguished), the relative weight of legal and extralegal factors shifted markedly. The basic underpinnings of the plurality opinions in *Furman* (and, later, *Gregg*) continue to govern the imposition of the death penalty (i.e., the Court has not yet reverted to a pre-1972 posture). That is why the legal model continues to provide a viable framework by which to assess Court outcomes. Yet the more political and politicized factors exert the greatest influence today on case outcomes.

Again, this is not surprising, given previous research on Court decisions in capital cases in particular (e.g., Zimring and Hawkins 1986). But, it does reveal something about the nature of judicial decision making. In short, as law governing capital punishment evolved, the Court moved away from the original doctrine per se and looked instead to the extralegal environment.

CONCLUSION

We began with a relatively simple purpose—to determine how well legal and extralegal models performed in head-to-head competition. We thought this a significant enterprise because theories of judicial behavior are rarely subjected to this sort of examination. As it turned out, both models performed satisfactorily, evincing strong abilities to account for Court outcomes in capital punishment litigation.

Our analysis could have ended here, concluding that legal and extralegal models provide equally good frameworks by which to assess Court decision making. Such a conclusion, however, would have been premature, because, upon further evaluation, we found that both exhibited disturbing propensities. The legal one prematurely anticipated liberal outcomes, the extralegal model conservative ones. Given these results, we tested another proposition, namely, that extralegal and legal frameworks presented codependent, not mutually exclusive, explanations of decision making. Based on these results, we offered an integrated model of judicial decision making contemplating a range of legal and extralegal factors.

Taken as a whole, what does our model reveal about the nature of Supreme Court decision making? Can we now address our original research question, How does the Supreme Court reach decisions? On some level, its answer is obvious: the most complete explanation of judicial outcomes should incorporate legal and extralegal factors. Seen in this light, the views of neither the classical legal thinkers nor the behavioralists are incorrect; but they are incomplete. Both law and politics, as we would intuitively expect, play significant roles in the Supreme Court's decision-making process.

On another level, however, these roles may not necessarily be equivalent. Our analysis of capital cases reveals that legal factors have the greatest impact at the early stages of an issue's life; as it evolves, however, extralegal variables dominate. In a way, therefore, our results reinforce the legal paradigm set out by the pragmatic Benjamin Cardozo (1921). When the law ceases to be consistent with the Court's "sense of justice," there occurs a greater willingness to abandon it. This, he wrote, *should* be the nature of the judicial process. Our analysis confirms that (at least for death penalty litigation) it *is*.

Where do we go from here? Surely our next research tasks must be to eliminate some of the shortcomings of this effort. In particular, our focus on a single area of the law hampers our ability to reach generalizable conclusions. Hence, future analyses ought to continue where we have left off by exploring other legal issues with an eye toward discerning whether the patterns we found are idiosyncratic or sufficiently universal.

Notes

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1. On the other hand, we could argue that this is more the "norm" than it is an anomaly; that is, research on the judicial process rarely compares models of decision making. Two of the few exceptions are Gibson 1978 and Johnson 1987. The latter, especially, is relevant to our study. Johnson sought to determine whether a legal or political model accounted for "more variation in the use of Supreme Court decisions by lower federal courts" (1987, 325). In the end, his analysis lends support to "those who argue that legal factors in part account for judicial behavior, especially in lower courts" (339).

2. For a vivid illustration, cf. Cushman's and Segal's treatments of search-and-seizure cases (Cushman 1929, 82–84; Segal 1984).

3. Kort corrected some of these problems in later works (e.g., Kort 1973).

4. Many textbooks and readers on the judicial process distinguish *fact pattern* analysis from legal models. As Carp and Stidham note, "While fact pattern scholars also believe that the facts of a case determine its outcome, they quickly part company with the traditional approach after that point" (1985, 193). Among the most important factors setting it apart from a legal model, according to them, is the use of legally irrelevant facts. In this light, Segal, who does not consider such "irrelevant facts," comes far closer to validating doctrinal perspectives than behavioral ones.

5. These studies focus on the U.S. Supreme Court. For an excellent example of the application of the legal model to lower-court decision making, see Johnson 1987.

6. Here we refer to probit—an analytic technique well suited to binary choice variables, such as judicial outcomes—because it operates on a continuous S-shaped probability function.

7. *Gregg* v. *Georgia* 1976.

8. *Coker* v. *Georgia* 1977.

9. *Enmund* v. *Florida* 1982.

10. *Gregg* v. *Georgia* 1976.

11. *Witherspoon* v. *Illinois* 1968; *Adams* v. *Texas* 1980.

12. See, generally, *Furman* v. *Georgia* 1972; *H. Roberts* v. *Louisiana* 1977; *Lockett* v. *Ohio* 1978.

13. *Woodson* v. *North Carolina* 1976.

14. *Furman* v. *Georgia* 1972; *Gregg* v. *Georgia* 1976.

15. *Godfrey* v. *Georgia* 1980.

16. *Estelle* v. *Smith* 1980.

17. We adopt this measure from Segal's analysis (1984, 895). But we are not unmindful of its flaws, the most important of which is that it assumes identical changes from one pair of justices to the next. For example, Segal treated the change from Warren to Burger as the equivalent of the change from Douglas to Stevens. Based on Baum's scores and rankings of the justices (1989b, 517), this is somewhat troublesome (i.e., Stevens and Douglas generally supported civil liberties claims, as did Warren; Burger generally did not). We face a similar problem; if anything, our predicament is somewhat more exaggerated than Segal's because, for instance, we are treating the change from a Roosevelt–Ford (Stevens for Douglas) pairing as the equivalent of a Nixon–Reagan (Kennedy for Powell) one.

We offer two reasons for so doing. The first, and more important, one speaks in general terms to this sort of research. Like Segal (1984), we wanted to assess the cumulative impact of ideology on institutional-level decision making and do so in a non–post hoc way. This approach allows us to reach that end in a far more satisfactory manner than would, say, indicators developed to tap the specific propensity of individuals. To demonstrate this, let us compare the bottom lines of Segal's (1984) approach to macro decision making (which we adopt here) with that which he and Cover fashioned in 1989 to identify a priori the ideology of individual justices. If we took the median ideology of the justices from Segal and Cover's (1989) scores, we would expect to observe *no* change in the Court's ideological propensity from 1975 to 1986 and a *decrease* in its overall conservatism from 1987 to 1988. In contrast, our adaptation of Segal's (1984) measure depicts a Court that is, generally speaking, growing more conservative over time. No question exists as to which, on its face, is the more valid measure.

We also, of course, are mindful of the fact that we are dealing with a particular area of the law. Accordingly, our second reason is more substantive and contextual. As we know, the death penalty did not become a major political issue and thus a consideration in the Supreme Court nomination process until *Furman* v. *Georgia* 1972, after Eisenhower and Nixon made their appointments. This is particularly critical for our treatment of the Reagan-to-Nixon appointments. While Nixon ensured that his nominees were law-and-order advocates, he had no reason to identify their position on death penalty. In contrast, later Republican presidents (particularly Reagan) included a pro–death penalty stance as a requirement for nomination (Schwartz 1988). Accordingly, it is possible that some of the Nixon appointees were less predictable on capital punishment than on general issues of criminal law. (In fact, Epstein and Kobylka 1992 empirically demonstrates it). In short, at least in the death penalty area, we have every reason to suspect that a "conservative" justice was potentially replaced by an even more "conservative" one.

18. The office of the solicitor general never participated as a party, nor did it file any briefs in support of the defendants' position.

19. In the interest of space, we generally limit our discussion to the results uncovered from the probit analyses. For readers seeking more information, we would be most willing to provide copies of the data set. Contact Epstein by post or bitnet (C38054LE @ WUVMD).

20. Brenner, Hagle, and Spaeth (1990) offer the formulation

$$\text{ROE} = 100 \times \frac{\text{\% correctly classified} - \text{\% in the modal category}}{100\% - \text{\% in the modal category}}.$$

21. As is the case for all analytic techniques (e.g., ordinary least squares regression), researchers must concern themselves with interpretive problems that can result from violating the assumptions of the extant underlying statistical theory. In particular, we must at least consider the problem of serial correlation. As Aldrich and Nelson note, "When residuals are serially correlated, maximum likelihood estimates remain unbiased in large samples, but they are not efficient. The result is precisely analogous to that of autocorrelated linear models" (1984, 81). Yet we will not statistically address the issue of serial correlation for the same reasons offered by Marra, Ostrom, and Simon (1989, 605–7). Most important is that "there is no agreed-on test for statistical significance in the nonlinear context" (ibid.). Aldrich and Nelson concur, "Unfortunately, corrections for serial correlation have proven untractable in the logit and probit cases" (1984, 81). Adding to our decision was the fact that like Marra, Ostrom, and Simon (1989, 605), our data lack a constant time interval between observations.

22. To determine this, we removed DQ from the model, reestimated it, and compared the log-likelihood ratio it produced with that listed in Table 1. Overall, the removal of DQ resulted in a significant (at .05) reduction in the model's explanatory power.

23. We stress this point because of the possibility that conceptual overlap between the court change and political environment variables bled into our analysis. After all, some scholars argue that it is through the appointment process that Congress and the president primarily affect the Court. Several diagnostic checks, though, confirm our finding that the variables have independent and significant effects. We first examined whether they were significantly correlated; they were not. We then checked for an interaction effect. We added to the extralegal model an interaction term (PE × CC). While both CC and PE produced significant estimates, the interaction term did not.

24. More specifically, Ulmer addressed the question of whether social background models are time-bound, concluding that, indeed, they "work only for particular sets of judges at particular points in time" (1986, 965). A more recent study by Tate and Handberg (1991) argues that there may be no way to avoid this problem completely. However, they suggest that if researchers develop more theoretically sensitive personal attribute models, the problem of time-boundness can be minimized.

25. They are Natural Court 1 (1971–74), Natural Court 2 (1975–80), Natural Court 3 (1981–86), and Natural Court 4 (1987–88). Natural Courts are periods during which the Court's membership remains stable. We should note, though, that Natural Court 3 is not a pure Natural Court since a membership change did occur during the 1986 term: Scalia replaced Rehnquist, who ascended to the chief justice position left open by Burger's retirement. For purposes of presentation, we grouped 1986 with Natural Court 3.

26. To be sure, then, the categorization accuracy of the extralegal model remained high (91%), but had we merely guessed that all 1989 term cases would result in pro-state outcomes, we would have been equally accurate. Moreover, if these categorizations result from trended behavior (as we suspect), we cannot necessarily conclude that the model is explaining Court outcomes.

27. In *Hildwin*, the Court held that the Sixth Amendment does not require that a jury specify the aggravating circumstances authorizing the imposition of death—that they could be specified by a judge. One of the statutorily allowed aggravating circumstances was that the killing was "especially heinous, atrocious and cruel." The Court concluded that the validity of this circumstance did not need to be addressed, since the imposition was based on other factors provided by statute.

28. In *South Carolina* v. *Gathers* 1989, the Court considered the remarks of a prosecutor at the sentencing stage, remarks that the state supreme court held "conveyed the suggestion appellant deserved a death sentence because the victim was a religious man and a registered voter." The Supreme Court agreed, ruling that "our capital cases have consistently recognized that 'for purposes of imposing the death penalty . . . [the defendant's] punishment must be tailored to his personal responsibility and moral guilt,' citing *Enmund* v. *Florida* 1982 (ibid., 2210).

29. Another explanation centers on how we measured the Court's internal ideological disposition. It is possible that the *Court change* variable, which increases over time, may be responsible for the extralegal model's categorizations. To explore this possibility, we reestimated the model using the means of Segal and Cover's (1989) values instead of our CC measure. As we have pointed out (n. 17), their scores are less than ideal for macro analysis. Therefore, we did not adopt them; and the reestimation reflects this deficiency. More specifically, the results, through Natural Court 3, parallel those we obtained. The model does a satisfactory job in categorizing cases but by Natural Court 3, overestimates conservative outcomes. For Natural Court 4, however, it correctly categorizes only 50% of the cases, a percentage below either of our models for any Court era. Such is hardly surprising, given that Segal and Cover's mean values anticipate that Natural Court 4 would be less conservative than that which preceded it. As a result, we cannot reach any firm conclusions; we can only offer the explanation in text as a strong possibility.

30. Of the eight missed cases, five were liberal outcomes, and three were conservative ones.

31. To create the instruments, we followed the approach outlined by Ostrom and Job, reestimating the equations and then using the standard deviation of the underlying scale and of each instrument to compute the beta weights (1986, 565).

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THE POLITICAL CONSEQUENCES OF INTOLERANCE: CULTURAL CONFORMITY AND POLITICAL FREEDOM

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I demonstrate that the intolerance of ordinary citizens matters for real politics even if strong linkages to policy outputs do not exist. In particular, the model I test posits that cultural intolerance constrains the liberty of individual citizens. Focusing on how people perceive political freedom, several hypotheses coupling tolerance and freedom are explored. Data from a national survey show that tolerance and freedom are connected. Those who do not feel free to express themselves politically are more likely to be intolerant of others, to have less heterogeneous peer groups and less tolerant spouses, and to live in less tolerant communities. Ultimately, the importance of mass political intolerance in the United States is that it establishes a culture of conformity that seems to constrain individual political liberty in many important ways.

When scholarly inquiry into political intolerance began nearly 40 years ago, there was little doubt about the political relevance of mass commitments to democratic values. Having just survived a world war against authoritarian dictators who seemed to have been abetted by their citizenries, there was widespread concern about whether democracy could exist in the face of the antidemocratic tendencies of the people. Just as the authoritarian movements of Europe and the Far East seemed to find succor in the hearts and minds of the people, the antidemocratic movement of McCarthyism threatened liberty in the United States. Few would have doubted whether citizen beliefs about majority rule and minorities rights mattered for politics.

Though 40 years of research has consistently shown a deep strain of intolerance in U.S. mass political culture,[1] there is now a growing concern about whether these research findings have any real political implications. It is clear, for instance, that mass political tolerance is not a prerequisite to democratic government and that relatively democratic regimes can exist even when citizens hold fairly antidemocratic beliefs. Indeed, some have even suggested that the intolerance that we have so long measured and attempted to explain (both in terms of its consequences and etiology) has no meaningful existence in the first place (Mueller 1988). Absent any clear understanding of whether mass political intolerance does or does not matter, there is a growing uncertainty about the utility of further inquiry into the democratic beliefs of ordinary people.

Consequently, it is important for those who study mass political intolerance to move beyond simple descriptions of political beliefs and to demonstrate the political relevance of the antidemocratic values of citizens. I offer a theoretical means of linking these attitudes with politics. The linkage hypothesized is not through the traditional opinion–public policy process but is, instead, directed at the mass political culture. I focus on *perceived political freedom*—how people view the availability of liberty. My basic hypothesis is that intolerance matters for politics by constraining the freedom available to ordinary citizens. On the basis of this theoretical framework, survey data on the political relevance of political intolerance are presented, and several hypotheses are tested.

In particular, I consider this problem of cultural intolerance using three different analytical strategies: (1) I link intolerance and freedom within the minds of individual survey respondents; (2) I examine how intolerance within the peer group (especially the family) constrains political freedom; and (3) I posit that lack of freedom is a function of local cultural values and community sentiments. Throughout much of this investigation, I employ a contextual form of analysis.

Each of the analytical strategies I pursue is assuredly incomplete. None of the analysis is irrefutable. My objective is to explore some of the ways in which cultural intolerance impinges upon the beliefs and behaviors of ordinary citizens. At the risk of some disjointedness, I am therefore presenting three different (but related) processes through which intolerance constrains freedom. The strength of the argument presented here is most fairly judged by the cumulative picture that emerges, not by the results of the tests of individual hypotheses. In light of the paucity of research on the *implications of intolerance*, this incomplete, but broad, research design is preferable to a comprehensive but narrow one (which is at the moment impossible anyway). In keeping with my desire to move beyond a simple micro analysis of intolerance, I close with some observations about the implications of these findings for a broader theory of the role of public opinion in democratic politics and the allocation of political freedom.

CONVENTIONAL UNDERSTANDINGS OF THE RELEVANCE OF MASS POLITICAL INTOLERANCE

The initial impetus for the empirical study of the democratic beliefs of citizens was largely provided by democratic theorists (e.g., Griffith, Plamenatz, and Pennock 1956). The mass-based totalitarian political movements of the period of World War II and its aftermath led scholars to examine more closely the nexus between democratic political institutions and the beliefs of citizens (e.g., Almond and Verba 1963; Lipset 1960). The debate that ensued was structured around the issue of whether citizen commitments to democracy were a prerequisite to democracy. The assumption that the survival of democracy as a process requires citizens to hold certain values received quite a jolt from the empirical evidence from surveys of the beliefs of ordinary people (e.g., McClosky 1964; Prothro and Grigg 1960). Citizens of the United States in the 1950s and 1960s (assuming this to be a democracy) were found to be quite intolerant of political nonconformists.

Out of a variety of empirical survey evidence was born the *elitist* theory of democracy. This theory, which has many variants,[2] accepts as an empirical fact the intolerance of the mass public but argues that under most circumstances this intolerance is neutralized. Except under extraordinary political conditions, the antidemocratic mass public is immobilized by its own ignorance and apathy, leaving the relatively more democratic elite free to rule in a democratic fashion. This theory argues that mass opinion is important, since in times of political crisis the masses can be mobilized by antidemocratic elites, but that during times of ordinary politics these beliefs have few direct political consequences.

Studies that attempt to test hypotheses linking public opinion and public policy on matters of political tolerance and democratic rights have been rare, however.[3] Focusing on changes in public opinion and their impact on public policy, Page and Shapiro (1983) discovered that in eight of nine policy changes in the area of civil liberties, there was congruence between public policy and public opinion. What the public generally wanted, it seems to have got. On the other hand, I investigated the hypothesis that mass political intolerance was connected to the adoption of repressive public policy by the states during the McCarthy era (Gibson 1988) but found no such relationship. Nor is there evidence that intolerant mass opinion directly spurred the state legislatures to adopt repressive statutes against dissent on university campuses during the Vietnam War era (Gibson 1989a; see also Gibson and Tedin 1988). This research, though limited in many important ways, cast some doubt on the elitist theory's presumption that mass intolerance causes outbreaks of political repression during times of political crisis. Nonetheless, it is important for my concerns here because it *fails to identify any direct political consequences flowing from mass opinion*.[4]

Absent direct linkages to public policy, is mass political opinion irrelevant? Perhaps not. Public policy is not the only dependent variable worthy of consideration. Even when opinion does not directly set policy, it may nonetheless constrict the choices of policymakers by setting broad limits on acceptable policies. And even when government is unresponsive to citizen preferences, citizens may well constrain the actions of their fellow citizens. Indeed, the political life of a community may be strongly influenced by the beliefs and values of its citizens even in the absence of direct citizen influence on public policy. Thus, it is worthwhile to consider other mechanisms through which mass political intolerance matters for politics.

THE CONTEXTUAL–CULTURAL RELEVANCE OF POLITICAL INTOLERANCE

Perhaps the true significance of mass political intolerance lies in its contribution to the creation of a culture of political conformity, a culture in which political liberty is limited by the intolerance of ordinary citizens. In this sense, mass beliefs about the political rights and liberties of those with unpopular political views circumscribe how citizens interact with one another (see Chilton 1988; MacKuen 1990). The political relevance of political intolerance, then, can be found in the constraints on political thought and action that citizens impose *upon each other*. Thus, I hypothesize that people learn from the political culture that intolerance is widespread, that it is acceptable, and that there are tangible risks to asserting views that the intolerant culture finds objectionable. Political intolerance can thus define the context of politics for many citizens.

This suggests that the relevance of intolerance must be understood through a contextual analysis of the political culture. Those who study public opinion and electoral behavior have long been concerned with contextual effects[5] on opinion formation and political action, and there seems to be fairly widespread agreement that individuals form their opinions not in isolation but in interaction with their social environments. For instance, according to McPhee's (1963) theory of social reality testing, individuals consume information from their environments, develop personal understandings of the information, and then test hypotheses through interpersonal interaction (see also Huckfeldt and Sprague 1988, 1989, 1991). Through this reality-testing process, pressures toward conformity occur. It is rare that individuals reject the reactions of their peers. Though some scholars (e.g., Noelle-Neumann 1984) view this as a coercive process in which social isolation is imposed on recalcitrant nonconformists, individual citizens may conform through what they perceive as more or less voluntary processes.

Many have thought about contextual effects of this sort in terms of short-term attitude change and change in announced voting intentions and behavior.[6] While one can imagine that there are such effects on these attitudes and actions, politics is rarely salient enough for people to engage in continuous reality testing. The stronger effects are likely to be in terms

TABLE 1

Levels of Tolerance, United States, 1987 (%)

	PERCENT BELIEVING GROUP SHOULD BE ALLOWED TO ENGAGE IN ACTIVITY			
ACTIVITY PROPOSED	MOST DISLIKED GROUP	2D MOST DISLIKED GROUP	3D MOST DISLIKED GROUP	4TH MOST DISLIKED GROUP
Run for office	26.6	31.2	34.1	35.7
Legally exist as a group	31.5	36.7	37.6	42.1
Make a speech	49.5	38.2	48.3	50.3
Hold public rallies	32.6	34.5	36.3	39.6
Teach in schools	18.3	21.1	—	—
Make phone calls untapped	63.4	61.1	—	—
All activities	6.3	8.0	18.3	13.3

Note: Approximate weighted Ns: most and 2d most disliked, 1,205; 3d most disliked, 1,065; 4th most disliked, 655. The last two items in the table were not asked of the 3d and 4th most disliked groups.

of more basic political orientations and change over longer periods of time. In no area of political life are these social interactions more important than in the area of conformity and political tolerance.[7] In arriving at political beliefs and behaviors, citizens continually test hypotheses about what is and what is not acceptable within the cultural context. The range of acceptable beliefs varies in conjunction with variation in political culture, in conjunction with variation in levels of political tolerance that individuals experience.[8]

It is useful to distinguish between the macro and micro environments of the individual. The macro environment is the larger social and political community within which the respondent resides, while the micro environment is made up of the specific friendship and family groups relevant for the respondent. My guiding hypothesis is actually quite simple: I expect that more political freedom is perceived where these environments are more tolerant and that the environments are the most tolerant when they are the most diverse politically.

Thus, the challenge for research on political intolerance is to demonstrate the *cultural* relevance of these antidemocratic beliefs. Is there a culture of conformity in the contemporary United States? Is Tocqueville's observation, "In America the majority raises formidable barriers around the liberty of opinion" (Tocqueville 1948, 264), a valid assessment of contemporary levels of perceived freedom? Do individuals hold views that they perceive to be unpopular with their fellow citizens? Are they reluctant to express those views? To what degree is there self-censorship in the United States as a result of this perceived political intolerance? I will attempt to address these questions by testing the general hypothesis that mass political intolerance creates a culture of conformity and that this culture significantly affects citizens' perceptions of the freedom that is available to them.

INTOLERANCE AND PERCEIVED POLITICAL FREEDOM IN THE CONTEMPORARY UNITED STATES

Before considering the specific hypotheses, I turn for a moment to the conceptualization and operationalization of the key tolerance and freedom concepts. *Political intolerance* is a concept that has attracted considerable attention from public opinion scholars. *Perceived political freedom* has not. Thus, after a rather cursory review of the measurement of *intolerance,* I will consider in some depth how *freedom* is conceptualized and operationalized.

Levels of Political Intolerance

To what degree are Americans today politically intolerant? Conceptualizing tolerance as "opposition to state actions that limit opportunities for citizens, individually or in groups, to compete for political power" (Gibson and Bingham 1985, 106), we can turn to data from a 1987 survey of the mass public to address this problem (see Appendix).

Table 1 reports willingness to tolerate deviant and unpopular political groups. The data are based on survey items that require the respondents to select the specific political groups that are to be tolerated or not tolerated.[9] The table reports reactions to the four groups in American politics that the respondent most dislikes.[10] Several conclusions are warranted from the data. First, intolerance is quite widespread. This is demonstrated both by the responses to individual items and the overall unwillingness to allow these unpopular political minorities full rights of democratic citizenship. Indeed, it is remarkable that only 6% of the American people would allow the group they most dislike to enjoy the same political rights and opportunities that the rest of the polity enjoys. It is difficult to imagine how intolerance might be more widespread.

Nor is intolerance limited to a single extreme group. Rather, it is generalized across groups that are greatly (even if not extremely) disliked. Though there is some variation in intolerance in Table 1 that is related to the degree of negative affect toward the group, the relationship is neither monotonic nor strong. People probably tolerate groups they only a little dislike (e.g., Democrats probably tolerate Republicans); but it appears that even moderately intense antipathy often translates into intolerance.

Thus, there is little support for full civil liberties for unpopular political minorities. But these data still do not address the question of whether mass political intolerance matters. Does intolerance contribute to restrictions on freedom, self-censorship, and political conformity?

Levels of Perceived Political Freedom

How strongly does the political culture of the United States encourage and reward conformity and discourage and penalize political nonconformity? Perhaps the most appropriate strategy for considering perceptions of political intolerance would be to survey groups that are the contemporary targets of intolerance, such as atheists, Klansmen, communists, fascists, and homosexuals. Such a research design would be difficult to implement (but see Gibson 1987a on the views of homosexuals). An alternative design would be one in which respondents are asked to report on their own perceived political freedom.

According to Preston (1982), people cannot be free unless they perceive opportunities for self-expression, unless they have the capacities and conditions for deliberative choice.[11] To the extent that some citizens perceive constraints on their freedom, liberty is diminished. This view of freedom is useful because it implies that a key element of freedom is the *perception of the ability to make such choices freely*.[12]

This conceptualization of freedom is also salutary because it renders the concept susceptible to empirical investigation through an assessment of the perceptions of people of the conditions for deliberative choice. Most basically, do people perceive such conditions as available to them? Certainly, those who expect to endure sanctions for political activity have neither the capacity nor the conditions for free action. Though freedom is more than merely perceptual, the beliefs of individuals are important for understanding their own levels of real freedom.

Just what sorts of activities are of concern when we speak of "freedom to act"? Though many domains of activity may be of interest, my concern here is limited to *political* freedom, mainly within the context of liberal democratic polities. Relying on Dahl's (1971, 1989) theory of polyarchy, I postulate that in a liberal democracy, citizens must have the opportunity to communicate with their fellows and to join together in political parties and activist groups to press their political demands. Thus, from the perspective of democratic political systems, political freedom includes institutional guarantees of the right to engage in oppositionist political activity by speaking, assembling, organizing, proselytizing, and competing for political power.[13] From the perspective of the freedom of individual citizens, my concern is whether individuals perceive opportunity to express themselves about political matters.[14]

Within this conceptualization of perceived freedom, I recognize two primary sources of constraints on liberty: the larger political system and the immediate interpersonal context.[15] Citizens may perceive that public expressions of their political views are circumscribed by the laws and actions of the state. This external censorship is more consistent with traditional notions of political repression. In addition, however, both public and private expressions may be constrained by interpersonal pressures toward conformity and sanctions for deviance. Local networks of friends, family, and associates may impose substantial restrictions on their freedom. To the extent that the political culture to which the individual is exposed does not reinforce political diversity and respect nonconformity, individuals with unpopular views may perceive significant repercussions for expressing their opinions. Thus, in understanding personal political freedom, we must be sensitive to constraints emanating from the larger political system, as well as from the personal network of the individual.[16]

Perhaps the easiest to measure of these dimensions is perception of what political activity the government will permit. The top of Table 2 reports responses to the question whether the government would allow the respondents to engage in certain types of political activities, prefaced by the premise "Suppose you felt very strongly that something the government was doing was very wrong and you wanted to do something about it." The responses were collected through a four-point response set, though Table 2 reports percentages based on collapsing the categories *probably not allow* and *definitely not allow*.[17] Perceptions of freedom are closely related to the race of the respondent (Gibson n.d.[b]). Because of this and because the survey included a remarkably large and representative oversample of African-Americans (see Appendix), the data are reported separately for blacks and whites.

Table 2 reveals that significant numbers of Americans perceive that the government would *not* allow them to express their opposition to government policy through conventional and unconventional political activity. Racial differences on these items are dramatic. Blacks are *much* more likely to perceive constraints on their freedom than are whites. Perhaps most surprising is the substantial numbers of both blacks and whites who feel that the government would not allow them ordinary and quite conventional means of political participation. One-quarter to two-fifths of white Americans believe that the government would prohibit them from expressing their

TABLE 2

Perceived Political Freedom, Black and White Mass Publics

PERCEPTION OF FREEDOM	BLACKS	WHITES	FACTOR LOADING[a]
Believe government would *not* allow them to[b]			
Organize nationwide strike	85.9	79.0**	.49
Organize public meetings	63.7	39.5**	.88
Organize protest marches/demonstrations	54.5	35.3**	.85
Make speech criticizing government actions	55.6	29.6**	.78
Publish pamphlets	53.0	28.1**	.86
Are reluctant to talk about politics because they[c]			
Don't like arguments	58.3	46.5**	.58
Don't want to create enemies	42.3	37.3	.61
Worry that people would think their views strange	28.7	18.7**	.59
Worry what people would think of them	18.2	12.5**	.62
Worry the government might find out	12.5	2.4**	.44
Unwilling to[d]			
Put sign in front of home/apartment	71.0	77.2*	.66
Put bumper sticker on car	59.5	68.8**	.75
Participate in demonstration	55.7	61.8*	.65
Wear button to work or in public	51.4	55.8	.83
Sign petition for publication in local paper	51.4	39.1**	.77
Write letter to elected representative	37.1	30.3**	.64

[a] The factor loadings are based on a combined analysis of blacks and whites.

[b] The question leader read: "Suppose you felt very strongly that something the government was doing was very wrong and you wanted to do something about it. Do you think the government would definitely allow, probably allow, probably not allow, or definitely not allow you to. . . ." The percentages shown collapse the *probably not* and *definitely not* responses. The approximate number of black respondents is 434; for whites, N = 796.

[c] The leader of each of the items is "I am sometimes reluctant to talk about politics because. . . ." Response choices were *true* and *false*. The number of black respondents exceeds 420, and the number of whites is at least 785.

[d] The question read: "Let's say you did have a political view that you knew would be very unpopular with others. Would you be willing to . . . ." The percentages reported exclude those who would not do the activity under any circumstances, regardless of the popularity or unpopularity of the view. The number of black respondents exceeds 404 and the number of whites is at least 749.

*$p \leq .05$ (racial difference of uncollapsed responses).

**$p \leq .01$ (racial difference of uncollapsed responses).

opposition through conventional speech and assembly activities. Among blacks, 53–64% believe the same. And although it is perhaps a bit fanciful for ordinary people to imagine organizing a nationwide strike, four in five black and white Americans believe that the government would prohibit such efforts. The level of perceived *governmental* constraints on political opposition is astoundingly high.

Perceptions of governmental limitations on political freedom represent an external form of censorship. Internal censorship, or self-censorship, can be measured by asking whether individuals are willing to express themselves politically when their views are unpopular with the majority. Consequently, I attempted to measure the beliefs Americans hold that might justify self-censorship. The respondents were given the opportunity to accept or reject a variety of statements that might explain reluctance to talk about politics with their families and friends. Each of these statements represents the belief that there are significant personal costs to be paid for expressing one's views. The middle of Table 2 reports their responses to these statements. Many of these responses suggest strong reservations about political expression. Politics is perceived as sometimes leading to arguments (58% of blacks, 47% of whites) and as creating enemies (42% of blacks, 37% of whites). There is also a substantial minority of respondents that worries about nonconformity. Generally, many believe that political discussions can be associated with unwelcomed repercussions from friends and family.

The racial differences observed in perceptions of the governmental constraints on political action are not as stark here; but neither are they trivial. Blacks are significantly more likely to fear expressing their views because people would think them strange, are more fearful of repercussions from the government, more censor themselves due to a distaste for political argument, and are more fearful about their reputations. Special note should be taken of the nearly 13% of the black sample that is reluctant to talk about politics due to a worry "that the government might find out about me." This is an astonishing level of concern among African–Americans and represents a very serious perceived threat to personal political freedom.

I also asked the respondents whether they would be willing to engage in certain sorts of activities to express views in spite of their being unpopular with others.[18] These items represent a form of behavioral self-censorship. The bottom of Table 2 reports the results from these questions. A substantial number of

Americans are reluctant to express unpopular political views. The figures range from roughly three-fourths who are unwilling to place a sign in front of their home to announce an unpopular view to one-third who would not write a letter to one of their elected representatives. It is not particularly surprising that large majorities would be unwilling to announce an unpopular view with a bumper sticker or a yard sign, since to do so makes one's property vulnerable to those who find the position offensive. But a majority of Americans are also unwilling to express an unpopular opinion through less risky activities such as wearing a button or participating in a demonstration. Indeed, nearly one-third of the American people would be unwilling to write a letter to an elected representative to express an unpopular political position! Only one-sixth of the respondents would engage in all six of the activities; fully 28% claim that they would do *none* of the activities. In general, the greater the direct personal accountability for the view, the less likely the respondents are to want to engage in the activity. Once more, we see that a substantial portion of the American people is fearful of public expression of unpopular political views.

Racial differences in behavioral self-censorship are less pronounced than on the other sets of items. Blacks are significantly less likely than whites to be willing to sign a petition that would be published in the newspapers; but for some activities, *whites* are substantially less likely to be willing to express themselves. For instance, nearly 10% more whites claim they would not put a bumper sticker on their cars (a difference that is significant). The differences in these frequencies suggest that the sources of self-censorship may vary according to race.

In order to examine the causes and consequences of perceived political freedom, it is useful to construct some indices of political freedom. I employ factor analysis to construct the measures, since in factor analysis the items are weighted unequally in the construction of the summary indices. Since all of the factor analysis results support the assumption of the unidimensionality of the measures, scores from the first unrotated factors serve as the specific indices. The factor loadings for each of the three factor analyses are also shown in Table 2.[19]

According to the results I have presented, political freedom is not widely perceived to pertain in the United States. Levels of perceived governmental constraints on freedom and self-censorship are surprisingly high. Moreover, perceived freedom seems to be allocated unevenly across racial groups. African-Americans perceive themselves as having less liberty, just as they have less of nearly everything else of value in American society. It remains to consider whether variation *beyond that associated with race* can be accounted for. I begin this inquiry with consideration of the connections between freedom and tolerance within the minds of the individual respondents.

CONNECTING FREEDOM AND TOLERANCE AT THE INDIVIDUAL LEVEL

To what degree do tolerance and perceptions of political freedom go together? Do those who are intolerant perceive more limited freedom for themselves? Figure 1 shows the bivariate correlations among the various indicators of perceptions of freedom and the index of political tolerance for both blacks and whites. Generally, the correlations do not differ substantially across race (although there are a few exceptions); and the magnitude of the black coefficients is generally somewhat smaller than the white coefficients. Beliefs about tolerance and freedom are fairly closely connected. There is a moderately strong relationship between political tolerance and perceptions of repression, with those who are more tolerant perceiving *less* political repression. These people apparently believe not only that they are free to act as they wish but that others should be free as well. Conversely, those who would deny liberty to their political opponents do not necessarily claim it for themselves. Those who are more tolerant are also less likely to engage in self-censorship. Whether tolerance flows from some sort of norm of reciprocity and fairness ("Because I am able to express my views, others should be allowed to express theirs") or individuals are projecting their own tolerance onto others cannot be determined. Tolerance of others is associated with the belief that there are few significant costs to be paid for one's own political self-expression.

It would be tempting to argue that perceptions of government constraints inhibit self-expression; and, indeed, there are significant correlations between these variables for both blacks and whites. Yet I am a bit reluctant to interpret the causal pathway. Those who self-censor may rationalize their behavior through beliefs about repression rather than limiting themselves out of fear of governmental retaliation. At the moment, it is prudent to conclude no more than that most Americans—black and white—hold reasonably consistent views about freedom and tolerance.[20] Tolerance matters because it is connected to a set of beliefs about the legitimacy and appropriateness of self-expression.

CONTEXTUAL ANALYSIS OF CONSTRAINTS ON FREEDOM

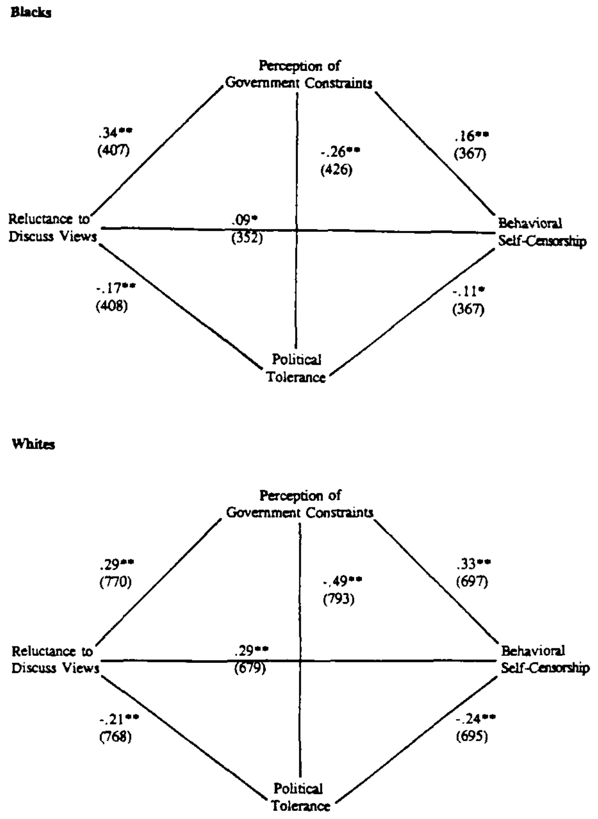
Does cultural intolerance inhibit freedom? While the analysis presented is consistent with that hypothesis, it does little to elucidate the causal pathways. For that, it is necessary to turn to somewhat different data about the interpersonal networks of the respondents.

Networks

The most direct test of the hypothesis that lack of freedom flows from cultural intolerance would re-

FIGURE 1

The Simple Interrelationship of Tolerance and Perceptions of Freedom, Black and White Mass Publics



Note: Entries are bivariate correlation coefficients. The numbers in parentheses are the numbers of cases on which the correlations are based.
$^*p \leq .05$.
$^{**}p \leq .01$.

quire that interpersonal networks be mapped and interviewed (e.g., Knoke and Kuklinski 1982). It would then be possible to correlate attributes of the network with perceptions of the individual. This sort of research design is extremely costly and difficult to execute,[21] and because many sanctions for nonconformist behavior are no doubt situationally determined, it would still only provide limited evidence of central tendencies. An alternative research design is therefore necessary.

Following the methodology of earlier research (e.g., Huckfeldt and Sprague 1991; Leighley 1990; MacKuen and Brown 1987), I will test the cultural hypothesis using respondent reports about their immediate set of peers. The respondents were asked to name three individuals with whom they "discuss important matters."[22] They were then asked to specify the party identifications of each of the people, as well as the frequencies with which they talk about political matters. Using these variables, an indirect test of contextual constraints on freedom can be conducted.

One of the most important attributes of the micro environments of individuals is diversity.[23] Social scientists have long argued that exposure to political diversity contributes to political tolerance (e.g., Nunn, Crockett, and Williams 1978). As one is exposed to a greater range of political ideas, one can become aware that alternative viewpoints are possible and legitimate and that one's own view may not be absolutely correct (see Duch and Gibson 1992). Political homogeneity generates and reinforces a closed-mindedness that is not conducive to political tolerance. As Blau noted: "The attenuation of profound social bonds that firmly integrate individuals in their communities is often deplored. But strong ingroup bonds restrain individual freedom and mobility, and they sustain rigidity and bigotry. Diverse intergroup relations, though not intimate, broaden horizons and promote tolerance, and they are the basis of macro-social integration" (1974, 623). Those with more diverse peers are expected to perceive a greater level of political freedom because group diversity nurtures political tolerance.

Using the respondents' reports on their networks, some indication can be got of the political diversity of these peer groups by comparing the party identifications of the respondents with their reports of their friends' party identifications.[24] An overall index of exposure to partisan diversity can be created by simply counting the number of members of the peer group that has perceived affiliations differing from the respondent's party attachment.[25] The index ranges from zero (no friends with different party affiliations) to three (all three friends are of a party different from the respondent's).[26]

Only a small majority (60%) of the respondents can identify the party affiliations of at least two of the three people in their networks. Of those who know the party of at least one of their friends, most report fairly homogeneous peer groups: 50% of the sample identifies no members of the immediate peer group with different party affiliations, while only 8% has three friends with differing party affiliations. This finding of relative network homogeneity is common in the research literature (e.g., Marsden 1987).

This measure of homogeneity can be further weighted by the frequency of political discussion; that is, each respondent was asked to indicate the frequency with which he or she discussed politics with each of the three peers. Thus an overall index score can measure the frequency of discussion of politics with people of differing partisan persuasions.[27] This is a more sensitive measure of exposure to political diversity. I hypothesize that greater exposure to political diversity is associated with greater political tolerance and with a greater sense of political freedom. Table 3 reports the analysis.

The direct relationship of network partisan heterogeneity and perceptions of freedom is tiny. The

TABLE 3

The Impact of Peer Group Heterogeneity on Political Freedom

PERCEPTION OF FREEDOM	PEER GROUP HETEROGENEITY	PEER GROUP HETEROGENEITY-POLITICIZATION
Blacks		
Perceptions of government constraints	−.01	−.11*
Reluctance to discuss political views	−.05	−.06
Behavioral self-censorship	−.00	−.14**
Whites		
Perceptions of government constraints	−.03	−.21**
Reluctance to discuss political views	.03	−.10**
Behavioral self-censorship	−.04	−.07*

Note: Entries are bivariate Pearson correlation coefficients.
*$p \le .05$.
**$p \le .01$.

correlations for blacks and whites are quite weak. Individuals who have more heterogeneous groups of peers perceive little more freedom available than those with homogeneous peer groups.[28] In light of the variability in political content and the frequency of discussion, this finding is perhaps not surprising.

When the diversity measure is weighted by the frequency of political conversations, the relationships are stronger and, in most cases, statistically significant. When political discussion is common within the peer group *and* the peer group is composed of members with differing party identifications, blacks become significantly less likely to engage in behavioral self-censorship or to perceive governmental constraints on their freedom. Under similar conditions, whites perceive fewer governmental constraints, are less reluctant to talk about politics, and engage in less behavioral self-censorship. Peer group diversity thus contributes to greater perceived freedom. Were a more direct measure of peer group tolerance available, these relationships would likely be stronger. Nonetheless, I take these data as supportive of the basic hypothesis that exposure to diversity legitimizes differences and thereby contributes to political freedom.[29]

This analysis concerns peer groups that are made up of a variety of types of individuals, each treated as having equal importance for the respondent. Peer groups are certainly of some importance for learning about the limits of political freedom. However, it is likely that people also learn lessons about freedom from their experiences within an even more micro environment, the family. In particular, I hypothesize that the political intolerance of a spouse has something to do with how one perceives political freedom. The available data allow at least an indirect test of this proposition.

Intolerance in the Family

For most married couples in the United States, spouses represent an important reference group. When asked to name three other people with whom important matters are discussed, nearly 76% of the married people in this national sample named their spouses. Fully 39% listed a spouse as *the* most important person. Moreover, there is considerable political content to the discussions that husbands and wives have. Nearly 25% of the sample report that they frequently talk to a spouse about politics, while only 14% claim that they talk quite rarely to a spouse about politics. It is reasonable to ask, therefore, to what extent the intolerance of a spouse contributes to perceptions that freedom is best not exercised.

The optimal research design requires that spouses be interviewed and be interviewed independently of their mates. Such a design is feasible (see Brickell, Huckfeldt, and Sprague 1988; Huckfeldt and Sprague 1989, 1991); but it is quite costly and difficult to implement. An alternative is to allow respondents to serve as informants on their spouses. Such a strategy probably cannot produce reliable direct information about spousal tolerance but can certainly be used to gather data about levels of education, religious affiliation, and so on.[30] Perhaps the data that can be reliably collected can be used to estimate the data that cannot be reliably collected.

The respondents in the 1987 survey were queried about a variety of attributes of their spouses. Fortunately, many of the demographic characteristics that can be faithfully reported in the survey are useful predictors of intolerance and can be used to estimate the intolerance of each respondent's spouse. Several steps (each involving important assumptions) must be taken in order to estimate spousal intolerance.[31]

The intolerance of the spring 1987 respondents was regressed on several demographic attributes of the subjects (see Table 4).[32] The regressions were conducted separately for the various combinations of race (blacks and whites) and gender. The sample was separated by gender in order to allow estimation of spousal intolerance. The separation by race was useful because such significant differences in intolerance (as well as the predictors of intolerance) exist by racial

TABLE 4

Equations for Estimating Spousal Tolerance (Married Respondents Only)

PREDICTORS OF POLITICAL TOLERANCE	UNSTANDARDIZED REGRESSION COEFFICIENTS			
	BLACKS		WHITES	
	MEN	WOMEN	MEN	WOMEN
Education	.228	.351	.747	.862
	(.207)	(.208)	(.090)	(.141)
Church attendance	−.539	−.186	−.349	−.452
	(.252)	(.287)	(.101)	(.108)
Occupational prestige	.059	.018	.026	−.005
	(.041)	(.053)	(.020)	(.024)
Religious fundamentalism	.229	.356	−.411	.186
	(.885)	(.865)	(.346)	(.378)
Family income	.158	.078	−.084	.391
	(.242)	(.219)	(.146)	(.165)
Intercept	3.671	1.621	1.387	−5.411
	(3.772)	(3.766)	(1.877)	(.2354)
Minimum pairwise N	67	66	269	224
R^2	.168	.095	.359	.308

Note: Entries shown in parentheses are standard errors. The minimum pairwise Ns shown are the unweighted numbers of cases.

groups. The data reported in Table 4 are unstandardized coefficients, allowing comparison across the samples; and they are derived from only the portion of the sample that reported being currently married.

It should first be noted that the demographic model explains a considerable amount of the variance in tolerance among whites but a much reduced amount among blacks. The tolerance of whites is fairly well predicted by their demographic attributes, while the tolerance of blacks is not. Moreover, there are large differences in the intercepts that vary by race (as well as by gender). The large amount of unexplained variance among blacks should make us cautious in using these equations to estimate spousal intolerance.

Among whites there are some important differences across gender in the predictors of tolerance. Religious fundamentalism contributes somewhat to intolerance among men but has a much reduced effect among women. On the other hand, greater family income enhances the tolerance of women but has little effect on men. The largest difference observed is in the intercept. When education, income, and occupational prestige are at their lowest and when church attendance and religious fundamentalism are at their highest, women are considerably more intolerant than men (and this seems to be true across races). The tolerance levels of men are also slightly more predictable than those of women.

It is not my purpose to present a fully specified model of gender and racial differences on the basis of the data in Table 4. The indicators available are not suitable to that task. Instead, I aim to use these characteristics to generate an *estimated spousal tolerance score*. Because the tolerance of blacks is so unpredictable by this equation, I will exclude them from further consideration. Using the coefficients reported in the table and each respondent's reports of the characteristics of a spouse on these five variables[33] (and assuming that all marriages are heterosexual and not interracial) estimated scores can be derived for the married portion of the sample. The scores are estimated from the equation that is appropriate for the gender and race of the spouse, and the values for the independent variables are the attributes of the spouse (as provided by the respondent). These estimates provide some indication of the level of intolerance of the spouse.[34]

As expected, the correlation of the tolerance of white couples is relatively high (r = .43). Tolerant women tend to marry tolerant men and vice versa. Relative to other characteristics of couples, this is not a strong correlation, however. For instance, levels of education of husbands and wives are correlated at .63, and frequencies of church attendance are related at .72. Though this no doubt reflects in part the estimation of spousal intolerance, rather than the direct measurement of it, there is some degree of heterogeneity within families in levels of intolerance.

It is not possible to sort out the direction of causality in these data. We cannot determine whether the intolerance of husbands is caused by their wife's intolerance, or vice versa. We can, however, test the hypothesis that spousal intolerance is associated with a sense of lack of freedom. Where one's spouse is more intolerant, I expect to see greater self-censorship and a greater sense of constraints on personal political freedom. Because there are important gender differences, the relationships must be considered

TABLE 5

The Impact of Spousal Tolerance on Political Discussions

POLITICAL DISCUSSION WITH FAMILY	SPOUSE'S TOLERANCE		
	LOW	MEDIUM	HIGH
Male respondents			
Percentage talking about politics "often" with family	19.1 (67)	16.1 (87)	42.9 (49)
Percentage reluctant to talk about politics with family	15.2 (66)	13.8 (87)	4.2 (48)
Female respondents			
Percentage talking about politics "often" with family	21.1 (37)	20.9 (91)	22.2 (72)
Percentage reluctant to talk about politics with family	20.5 (39)	15.7 (89)	12.7 (71)

Note: Figures in parentheses are the *total* numbers of cases upon which the percentages are based (white married respondents only).

separately for men and for women. (As before, the analysis is confined to whites.)

Table 5 reports the relationship between spousal intolerance and the frequency and nature of familial political discussions among whites. Two indicators are reported: the percentage of respondents reporting that they "frequently" hold political discussions with their families and the percentage who claim that they sometimes feel reluctant to express their political views to their families.[35] I hypothesize that spousal intolerance contributes to unwillingness to discuss politics openly.

Intolerance limits political discussions, but the findings vary significantly by gender. Among men, the intolerance of wives significantly affects not only the frequency of political discussion but the openness of discussion, as well.[36] Where wives are more tolerant, political discussions are more common, and husbands are more likely to express their true political views. Thus, these data support the hypothesis. However, the intolerance of women's husbands has virtually no effect on the frequency of political discussions and only a slight effect on the openness of conversations. The gender differences are fairly stark: 43% of the men married to tolerant wives often talk about politics with their families, while only 22% of women married to tolerant husbands discuss politics often. Whether under conditions of low spousal tolerance or high spousal tolerance, women are more likely to censor their political views than are men and thus are less likely to realize the benefits of increased tolerance within marriages. Thus (perhaps ironically), the tolerance of wives has a bigger impact on family political discussions than does the tolerance of husbands.

The frequency and content of political discussions is no doubt influenced in part by levels of interest and competence in politics. If gender differences exist, then they might account for the differences shown in Table 5.[37] Instead of pursuing these differences, it might be more useful to turn to the general beliefs respondents hold about personal political freedom and consider the correlations between spousal intolerance and general perceptions of freedom for men and women.

White men who are married to less tolerant women tend to perceive greater governmental repression ($r = -.37$, $p < .01$). The same is true of women who are married to less tolerant men ($r = -.35$, $p < .01$). To the extent that these views are derived from familial experience, men and women react similarly. Moreover, there is an *independent* impact of spousal tolerance on perceptions of government repression; that is, when these perceptions are regressed on the respondent's *own* intolerance and a spouse's intolerance, the latter variable has a significant and nontrivial effect on perceptions. The increment in R-squared is a highly significant .03; and beta is −.19 (s.e. = .04).

However, there are hints of some differences between men and women in terms of self-censorship of political talk (if not action). Spousal intolerance has some effect on the willingness of men to express their political views ($r = -.16$, $p < .01$) but a more substantial effect on the general willingness of women to express their views ($r = -.28$, $p < .01$). On the other hand, when it comes to expressing their political views behaviorally (which usually takes place outside the home, anyway), neither men nor women are much affected by their spouse's intolerance (men, $r = -.14$, $p < .05$; women, $r = -.11$, $p > .05$). In terms of general self-censorship, we see an effect on willingness to express opinions that is opposite of that already observed. The views of women are affected by their husband's intolerance, but the views of men are little affected by the intolerance of their wives.

Thus, we have something of a paradox. Men with less tolerant wives tend to have fewer and less open political discussions at home, but their overall levels of perceived freedom are little affected. On the other hand, women with less tolerant husbands are neither more nor less willing to express themselves at home but generally perceive less freedom. It appears from these data that the experiences at home of married white men are not generalized to politics at large, while the experiences of women at home are generalized. If the South Bend, Indiana findings of Brickell, Huckfeldt, and Sprague (1988) are generalizable, this may be due to the tendency of men to discount and devalue political discussions with their wives. (See also Weiner 1978, which also reports a greater effect of husbands on wives' partisan affiliations than vice versa.) This conclusion, though highly speculative, is worthy of further investigation.

Apparently, people do learn something of the limits of freedom from their experiences in their families. Perhaps, however, this is in part a function of larger political environment in which individuals and families exist. Hence, I will turn my attention to a different sort of contextual analysis.

Community Intolerance and Perceptions of Freedom

Contextual analyses also profit from moving beyond individual networks to the macro environment of the individual (e.g., MacKuen and Brown 1987). It is useful to consider whether intolerance in the larger social and political community within which the respondent resides affects levels of perceived freedom. To explore the impact of community intolerance we first have to consider some important measurement and aggregation issues.

The traditional approach to measuring political tolerance derives from Stouffer's (1955) pathbreaking study and involves asking respondents whether groups assumed to be unpopular should be allowed to engage in certain activities. The General Social Survey frequently includes 15 measures of this sort. The measures are structured around five groups and three activities. The group stimuli are (1) "somebody who is against all churches and religion," (2) "a person who believes that Blacks are genetically inferior," (3) "a man who admits he is a Communist," (4) "a person who advocates doing away with elections and letting the military run country," and (5) "a man who admits he is a homosexual." The activities are (1) making a speech in the community, (2) teaching in a college or university, and (3) such a person's book being in the public library. The responses are collected through a dichotomous response set.[38] Though these items have been criticized (e.g., Sullivan, Piereson, and Marcus 1982) and are certainly limited in important ways, they have been used repeatedly by scholars interested in political tolerance (e.g., McCutcheon 1985; Bobo and Licari 1989; and Sniderman et al. 1989). I have recently shown that the Stouffer measures are remarkably similar to the Sullivan, Piereson, and Marcus "least-liked" approach (Gibson n.d.[a]). Especially since the traditional left-wing targets of intolerance were expanded (in 1976) to include militarists and racists, the items are less susceptible to the critique of Sullivan, Piereson, and Marcus.[39]

A scale based on the overall responses to the items can be of some utility as a measure of community intolerance. The tolerance index is the mean of the responses to these 15 items.[40] The mean index score is .56, with a standard deviation of .33; and the index is extremely reliable (Cronbach's alpha = .92). Generally, these data show remarkable temporal stability.

In order to test the contextual hypothesis, a measure of the intolerance of the local community must be derived. There are two possible geographical units that could serve to define the local community: the primary sampling unit and the block (enumeration district). In the 1980s, the National Opinion Research Center (NORC) consistently used the same 84 primary sampling units. These are fairly large geographical units, consisting of counties, standard metropolitan statistical areas, independent cities, and (in New England) parts of counties. The blocks are much smaller than primary sampling units. The 1980 sampling frame included 562 of these units.[41] The advantages and disadvantages of using the block as the measure of community opinion are obvious: the block has immediacy for the respondent, but the block mean is based on few cases. The primary sampling unit means are based on many more cases, but the primary sampling unit may not impinge much on the individual respondent. In the analysis that follows, I will aggregate by both units, bearing in mind the limitations of each.[42]

To what degree is the intolerance of the community reflected in the intolerance of the individual respondent? We can address this problem by correlating the measure of individual intolerance with the mean tolerance score for the primary sampling unit and block. Note that the tolerance score for the individual is operationalized differently from the tolerance score for the community[43] and that the magnitude of cross-level correlations is invariably quite small. Furthermore, the tolerance scores of the individual respondents are not used in constructing the aggregate scores for the primary sampling units and blocks in which they live. Nonetheless, the correlation between individual tolerance and average tolerance within the block is .33 for whites and .19 for blacks. For average tolerance within the primary sampling unit, the correlation is .22 for whites and .12 for blacks. That people are more like their immediate neighborhood than the larger community is not surprising. However, the more interesting question is whether the tolerance of the community is associated with perceptions of freedom by individual residents. The data relevant to this hypothesis are shown in Table 6.

TABLE 6

Community Tolerance and Perceptions of Freedom

PERCEPTION OF FREEDOM	AVERAGE COMMUNITY TOLERANCE: PRIMARY SAMPLING UNIT	AVERAGE COMMUNITY TOLERANCE: BLOCK
Blacks		
Perceptions of government constraints	−.16**	−.23**
Reluctance to discuss political views	−.13	−.19**
Behavioral self-censorship	−.20**	−.14**
Whites		
Perceptions of government constraints	−.17**	−.28**
Reluctance to discuss political views	−.09*	−.18**
Behavioral self-censorship	−.10**	−.13**

Note: Entries are bivariate Pearson correlation coefficients.
$p \le .05$.
**$p \le .01$.

The correlations reported in Table 6 are quite supportive of the hypothesis. Among both blacks and whites, those living in more tolerant communities are considerably more likely to perceive political freedom. For instance, intolerance in the community is significantly associated with perceptions of greater repression and with less willingness to express one's political views. Less tolerant communities seem to be successful at limiting freedom. These coefficients are all the more impressive when it is noted that they are cross-level correlations, so that all within-community variance in levels of perceived freedom must be treated as error variance.

It is impossible to sort out the causal pathways linking community intolerance with self-censorship. It is possible, for instance, that the community creates a set of norms that legitimize intolerance, that communal intolerance gets reflected in individual intolerance, and that citizens would deny themselves freedom just as they would deny their enemies freedom. Other processes are equally plausible. The only statistical test that is helpful in trying to unravel the process is to determine whether community intolerance has an impact on perceptions of freedom *controlling for the intolerance of the individual*. Data relevant to this hypothesis are shown in Table 7.

Table 7 reveals that community intolerance *does* have some effect on perceptions of freedom beyond the individual's own intolerance. For instance, for blacks, the beta coefficient relating community (block) tolerance to perceptions of governmental constraints on freedom is a highly significant −.18. For whites, the coefficient is −.14 (which is also significant at less than .01). These effects are not large either, and they are generally stronger for the block-level data than for the primary sampling unit data. But since the statistical tests are so strongly biased against the hypothesis, I take this as important support for the proposition that intolerance in the community limits available freedom.

Thus, there is fairly persuasive evidence that those who perceive constraints on their freedom live in communities characterized by higher levels of political intolerance. I cannot be certain that perceptions of

TABLE 7

The Independent Impact of Community Tolerance on Freedom

PERCEPTION OF FREEDOM	PRIMARY SAMPLING UNIT	BLOCK
Blacks		
Perceptions of government constraints		
R^2	.08**	.10**
Beta, individual tolerance	−.24**	−.22**
Beta, community tolerance	−.12**	−.18**
Reluctance to discuss political views		
R^2	.04**	.06**
Beta, individual tolerance	−.16**	−.14**
Beta, community tolerance	−.12*	−.17**
Behavioral self-censorship		
R^2	.05**	.03**
Beta, individual tolerance	−.08	−.09
Beta, community tolerance	−.18**	−.12*
Whites		
Perceptions of government constraints		
R^2	.25**	.26**
Beta, individual tolerance	−.48**	−.45**
Beta, community tolerance	−.07*	−.14**
Reluctance to discuss political views		
R^2	.05**	.06**
Beta, individual tolerance	−.20**	−.17**
Beta, community tolerance	−.05	−.12**
Behavioral self-censorship		
R^2	.06**	.06**
Beta, individual tolerance	−.22**	−.21**
Beta, community tolerance	−.06	−.07

*$p \le .05$.
**$p \le .01$.

lack of freedom are caused by local cultural values encouraging conformity. To unravel the complex causality involved here is far beyond the limits of the data and research design. Nonetheless, where there is greater intolerance in a community, one is more likely to find citizens who perceive significant costs to the exercise of political liberty.

The analysis presented here supports several conclusions. First, it appears that the perceptions ordinary citizens hold of political freedom can, in fact, be rigorously measured. Second, there is a surprisingly low level of perceived political freedom in the United States. Third, large and very important racial differences exist in how much freedom is perceived, with African-Americans asserting that there are many more constraints on their freedom than do whites.

More important are the findings connecting perceptions of freedom and intolerance. Not only are those who are intolerant more likely to perceive constraints on their own freedom, but I have also adduced evidence that intolerance in the external environment contributes to a lack of perceived freedom. More homogeneous (and presumably less tolerant) peer groups, less tolerant spouses, and less tolerant local communities all seem to limit how much freedom ordinary Americans perceive. One reason why perceived freedom is so low seems to be that intolerance is so pervasive within American political culture.

There are many important limitations to this research; and as a consequence of the limits of this research design, care must be taken in ascribing too much certitude to the conclusions. Nonetheless, there is a strong suggestion of a close connection between how people think about their own freedom and what freedom they would grant to their political enemies, as well as some more inferential evidence of a linkage between cultural intolerance and perceived limits on political freedom.

IMPLICATIONS

Does political tolerance matter? The cumulative weight of evidence from this analysis suggests that tolerance does matter. Though I have taken some risks in these analyses—with the consequence that no single bit of evidence is irrefutable—the data point repeatedly toward a connection between intolerance and lack of freedom. I have traced this relationship at the level of the individual, the family, and the community. Intolerance in American political culture appears to set at least broad constraints on political liberty. Those who study political tolerance can therefore take succor in these findings. It appears that we have not devoted our research efforts to analyzing attitudes of little or no political consequence.

At the same time, however, my analysis points to an entirely new frame of reference for tolerance research. No longer should we focus exclusively on the individual survey respondent as the most interesting unit of analysis. Why people differ in their levels of intolerance—and with what consequences—cannot be well understood by conceptualizing the individual in social isolation. John Donne's "No man is an *Iland*, intire of it selfe; every man is a peece of the *Continent*, a part of the *maine*" is an important and quite relevant prescription for social scientific research on the implications of political intolerance.

In Noelle-Neumann's "spiral of silence" people are encouraged "either to proclaim their views or to swallow them and keep quiet until, in a spiraling process, the one view dominated the public scene and the other disappeared from public awareness as its adherents became mute" (1984, 5). Perhaps more relevant to the case of the United States, Tocqueville observed many years ago, "I know of no country in which there is so little independence of mind and real freedom of discussion as in America" (1948, vol. 1, p. 263). Survey evidence from the contemporary United States seems to support this view, and this is a problem of considerable consequence for democracies. Indeed, the loss of respect for dissent and nonconformity in nominally democratic regimes is perhaps one of the greatest threats to political freedom (see Duch and Gibson 1992).[44] Without a culture that legitimizes political opposition, those outside the centrist mainstream have few political opportunities. Ultimately, the political system loses its democratic vitality. If this is a consequence of mass political intolerance, then, certainly, intolerance matters.

APPENDIX: RESEARCH DESIGN

The analysis I have reported is based on a national survey conducted in 1987. The survey was an extension of the 1987 General Social Survey (GSS). The GSS is a nearly annual survey conducted by the NORC with funding from the National Science Foundation. The sample for the GSS is a full multistage probability sample of English-speaking adults living in the continental United States. The 1987 GSS was conducted in the spring, with a response rate of approximately 75%.

In June and July, the respondents in the 1987 GSS were resurveyed. Of the 1,466 subjects in the spring GSS, 1,106 were eligible to be interviewed. (The NORC reserved approximately 350 respondents for possible use in future panel studies. Those subjects were not eligible to be reinterviewed.) This subsample was selected randomly, within gender strata. Because the 1987 GSS cross-section had a relatively large differential nonresponse rate by gender, there is some gender imbalance in the pool of subjects eligible for the second-wave interview. Consequently, stratified random sampling was thought desirable. For the reinterviews, males and females were selected with equal probability. Approximately 87% of these subjects were reinterviewed. Most of these were in-person interviews although, because some subjects had moved since the earlier interview, some small percentage of the reinterviews were conducted by telephone.

The 1987 GSS also included a special oversample of 353 black respondents (i.e., beyond the 191 blacks in the main GSS sample). This sample was also a full probability sample. This means that extraordinary efforts had to be mounted in order to draw this supplementary sample. Roughly five thousand households nationwide were sampled and contacted in order to locate the black subjects. The response rate for the black respondents was 79%. All of these subjects were eligible for the reinterview, and reinterviews were successfully completed with nearly 90% of the original subjects. It should be noted that the black oversample also overrepresents females. Because the universe of the black oversample was selected for inclusion in the reinterview project, no sampling techniques could ameliorate this problem. Thus, interviews were completed with a total of 1,267 respondents. Since the sample is stratified by race, most analyses are conducted on weighted data.

Special thanks are due Tom Smith and Jim Davis, coprincipal investigators on the GSS, and the GSS Board of Overseers for their assistance on this project. Dick Rubin of the NORC was instrumental in bringing the reinterview survey to a successful conclusion.

Notes

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1. Highlights in a fairly voluminous literature on political intolerance include Barnum and Sullivan 1989; Bobo and Licari 1989; Lawrence 1976; McClosky 1964; McClosky and Brill 1983; Nunn, Crockett, and Williams 1978; Prothro and Grigg 1960; Sniderman 1975; Stouffer 1955; Sullivan, Piereson, and Marcus 1982; Sullivan et al. 1985. I would also mention my own work: Gibson 1989b; Gibson and Bingham 1985; and Gibson and Duch 1991a.

2. The elitist theory of democracy is actually an amalgam of the work of a variety of theorists, including Berelson, Lazarsfeld, and McPhee 1954; Key 1961; Kornhauser 1959; and Lipset 1960. The most useful analysis of the similarities and differences among the theories can be found in Bachrach 1967. For a popularized summary of the theory see Dye and Zeigler 1987 (see also Dye 1976). My more recent analysis of some of the propositions of elitist theory can be found in Gibson 1988, 1989a; Gibson and Bingham 1984; and Gibson and Duch 1991b.

3. On the other hand, the literature linking opinion and policy in other substantive areas is voluminous. For recent examples, see Erikson, McIver, and Wright 1987; Erikson, Wright, and McIver 1989.

4. However, I have suggested that a climate of tolerant mass opinion during the 1970s may have contributed to acceptance of dissent as a legitimate form of political action (Gibson 1989a). Dissent, in turn, was an important stimulus for repressive policy. Thus, mass opinion indirectly affected policy, but the direction of the relationship is ironic. Where opinion was more tolerant, there was *more* political repression. Moreover, even if opinion does not produce a specific expectation for public policy, it may set broad constraints on the ability of elites to implement their own preferences.

5. Sprague defines *contextual effect* as "variation in political behavior that depends, systematically, on properties of the environment within which that behavior is embedded" (1982, 99). MacKuen writes "The quality of political life produced by *individual* choices made in a *collective* context differs fundamentally from one that might be expected from a simple aggregation of individual preferences" (1990, 61; emphasis original).

6. There is a large and important literature on pressures toward political conformity, beginning in the 1960s, including Cox 1974; Fields and Schuman 1976; Finifter 1974; Krassa 1990; MacKuen 1990; MacKuen and Brown 1987; McPhee 1963; Putnam 1966; Segal and Meyer 1974; Weatherford 1982; and the many works of Huckfeldt and Sprague. For a review of the social psychological literature on conformity see Moscovici 1985.

7. In a brilliant formal analysis of the contextual constraints on political discussion, MacKuen (1990) has addressed many of the processes I consider here. The overall implication of his analysis is that "public, if not private, conformity is likely to occur even when many individuals are perfectly willing to encounter contrary views" (p. 85). The linchpin of his analysis is an individual propensity for political interaction he terms *expressivity*. Were one to pursue an empirical analysis of expressivity, it would surely look something like the perceptions of political freedom that I analyze here. Note also that MacKuen agrees about the sensitivity of political discussion to social environments (pp. 94–95, n. 5).

8. Wildavsky (1987) has argued a similar position. Rejecting the proposition that citizen preferences are exogenous variables for political analysis, he suggests that choices reflect variations in relationships to the political culture, that is, they emerge from social interaction. Individual choices are choices of culture, they are decisions that reflect fundamental orientations toward the shared values that legitimate different patterns of social practices: "Put plainly, people decide for or against existing authority" (p. 5).

9. This technique, often dubbed the "least-liked" approach, was developed by Sullivan, Piereson, and Marcus (1979, 1982).

10. For a more complete explication of the measurement and distribution of opinion see Gibson 1987b, 1989b. The objective of the measures was to insure that all respondents were asked whether they would tolerate political activities by a) the two groups they disliked the most *and* b) Communists and members of the Ku Klux Klan. These objectives produced a somewhat complicated question format. All respondents were asked whether they would tolerate their two most-disliked groups. Those who named Communists and Klansmen as their two most-disliked groups (in either order) were then asked whether they would tolerate their third and fourth most-disliked groups. Respondents naming neither the Klan nor Communists as among the two most-disliked groups were asked about these two groups, rather than their third and fourth most disliked groups. The rest of the respondents were asked questions designed to insure that they evaluated Communists and Klansmen at some point in the question sequence. The precise group stimulus depended upon the

configuration of groups named as first through fourth most disliked.

11. I recognize that a distinction is typically made between negative and positive conceptions of freedom (see Berlin 1958). The negative view of freedom argues that to free people is to remove restraints on their actions. Rights perform this function by guaranteeing broad areas of autonomous activity within which the individual can make free choices. The positive view of freedom emphasizes rational self-determination. Freedom is thus assessed not in terms of whether conditions for free choice prevail but, rather, by the objective rationality of the choice. Decisions that are seemingly unrestrained but irrational are not free choices. The distinction between negative and positive freedom has been subjected to a variety of attacks and has been much debated (e.g., MacCallum 1967). For my purposes, it is more important to distinguish between subjective (perceived) and objective freedom than positive and negative freedom. Preston's (1982) conceptualization allows me to do just that. There is a certain asymmetry to my argument. Perceived restraints on choices—perceptions that certain political activities will generate adverse repercussions—certainly are evidence of the lack of freedom, even if I am on less certain ground when I claim that the *absence* of perceived restraints is equivalent to freedom. Nonetheless, if the absence of perceived constraints is grounded in reality, this is an important component of positive freedom. Clearly, those who expect to be penalized for their political activity are not free. In this sense, my conceptualization of freedom is closer to the negative view than to the positive view.

12. For a more extended discussion of this approach to freedom, see Gibson n.d.(b).

13. There are, of course, alternative conceptualizations of democracy and of democratic freedom, and I certainly do not claim this as the only legitimate approach. The emphasis I place on democracy as procedure (without much regard for substantive public policy) must be born in mind throughout the analysis.

14. This view of personal political freedom is focused on the individual citizen. It looks directly toward the attributes and beliefs of individuals, as well as their interactions with their immediate environments, to gain an understanding of freedom. In adopting this approach, I do not deny the utility of alternatives (e.g., focusing on the attributes of cultures or polities). But my basic premises are that what people believe about their own freedom is important; that what they perceive about the opportunities for political expression and the costs associated with such expression has much to do with their exercise of freedom; and that it is useful to regard the degree of freedom available and the patterns of allocation of freedom within a polity as ultimately reducible to a study of the beliefs of individual citizens. I do not gainsay the utility of considering other sorts of freedom; and I shall later try to identify some linkages between the individuals and the larger social and political environment within which they function. But the approach here must be recognized from the beginning as a particular, and not necessarily inclusive, approach to the problem of perceived personal political freedom.

The most useful conceptualizations of freedom are embedded in larger theories of politics. The theory I employ is most generally liberal–democratic, with specific reference to Dahl's theory of polyarchy. This perspective emphasizes obstacles that individual citizens might perceive to frank political expression. This is not a freedom of unconstrained licentiousness. Instead, it is freedom to engage in political activity that is essential to the effective functioning of polyarchies. It is quite possible for others to conceive of freedom differently, emphasizing, for instance, the potential (whether perceived or not) of individuals to develop nascent capacities for expression.

As much as I might wish to avoid the issue, I also have to acknowledge the distinction between objective and subjective freedom. My approach is to study subjective freedom—the freedom that is perceived by individuals. A different sort of freedom (e.g., the degree of freedom acknowledged by law) might be measured more objectively (and as an attribute of polities, not of individuals). Within my conceptualization of the problem, *there is no necessary relationship between objective and subjective freedom*. People may perceive themselves as free even in the most repressive conditions. (That sort of "false consciousness' is not addressed in this research.) Alternatively, people may not perceive the freedom that is truly available to them. When I refer to *freedom* I refer to the freedom that exists in the minds and hearts of citizens, not to the freedom that exists in the law books or even in the broader culture.

15. Huckfeldt and Sprague (1989, 1201) have quite carefully distinguished between *contexts* and *networks*. For their purposes, it makes a great deal of sense to do so. I am a bit sloppier with the use of the term *context*, but I do use *network* as the respondent's peer group.

16. I recognize that irrespective of the freedom allowed by the larger political system or local networks, individuals may be unwilling to express themselves politically for completely psychological reasons. For instance, lack of self-confidence may lead people to be unwilling to risk the opprobrium of others (Bay 1970 terms this *psychological freedom*). Though the effect of psychological attributes might be interactive (in the sense that personality attributes structure perceptions of, and reactions to, external environmental stimuli), the effect may exist completely apart from the objective attributes of the environment. This sort of psychological freedom is not addressed in this research. Instead, the psychological attributes of individuals are treated as independent variables, as predictors of social and political freedom (see Gibson n.d.[b]). Thus, rather than asserting that psychologically based risk-aversion is a sign of unfreedom, I prefer to test the hypothesis that those who perceive less freedom are more risk-averse and psychologically insecure. These hypotheses are addressed in Gibson n.d.(b).

17. Those who "don't know" whether the government would allow them to engage in the activity might legitimately be included with those believing that they would not be allowed to do so. Since the *don't knows* are quite few, I have simply excluded them from the calculations of the percentages.

18. Note that these figures exclude those who would not engage in the activity under any circumstances, regardless of the popularity or unpopularity of the view. For most of the actions, this number is inconsequential. However, 44 subjects say they would not put a bumper sticker on their car under any circumstances, and 29 would not wear a political button under any circumstances.

19. The first component of freedom is the perception that the government would allow political activity. The first unrotated factor from the common factor analysis had an eigenvalue of 3.4 and accounts for 68% of the variance in the items. The second factor that emerged from the analysis had an eigenvalue of .72 (explaining just 14% of the variance) and was therefore ignored. As indicated by the factor loadings reported in the table, the items contribute roughly equally to the scale, with the exception of the low-variance item on nationwide strikes. This index is quite reliable (alpha = .88) and can serve as a measure of perceived external governmental censorship of political behavior. As expected, racial differences on this index were extremely significant, with blacks perceiving substantially more political repression.

The index of perceived social censorship of political expression was derived by factor-analyzing the items shown in the second portion of the table. The first unrotated factor had an eigenvalue of 2.3 and explained 46% of the variance in the items. The second factor, which was not used, had an eigenvalue of .93 (accounting for 19% of the variance). This index is quite reliable, with an alpha of .70; and only the item about the government is at all weakly related to the factor. On this index, blacks are significantly more fearful of political expression than are whites. An index of behavioral self-censorship was also constructed. The reliability of this index is also quite high (alpha = .86), and all of the items contribute roughly equally to the scale. The first unrotated factor had an eigenvalue of 3.6 and explained 59% of the variance in the items. The second factor, which was not used, had an eigenvalue of .84 (accounting for 14% of the variance). The differences in the loadings across individual items are not of any substantive significance. Racial differences on this index are *not* significant. A fairly detailed analysis of the validity and reliability of these scales is available on request.

20. An alternative explanation for these findings is that the correlation between perceptions of freedom and tolerance is spurious, reflecting a common psychological origin. Consequently, it is useful to control for some additional variables before concluding that freedom and tolerance are linked. Three psychological attributes closely linked to political intolerance are closed-mindedness, or dogmatism (e.g., Gibson 1987a; Gibson and Tedin 1988; Sullivan, Piereson, and Marcus 1982), low self-esteem (Sniderman 1975), and anomie (Herson and Hofstetter 1975; McClosky and Schaar 1965; Quinley and Glock 1979; Zalkind, Gaugler, and Schwartz 1975). Each of the measures of tolerance and freedom is fairly strongly related to some personality attributes. The best predictor is dogmatism. Those who are more closed-minded are more intolerant, more likely to perceive government repression, and more likely to engage in self-censorship. This is true for both blacks and whites. Self-esteem has only a weak direct effect on tolerance, but is related to perceptions of government repression and willingness to express oneself in political conversations. This is probably due to the facility with which those high in self-esteem interact with others. Those who are high in anomie tend to be more intolerant and perceive more repression and are slightly more likely to engage in self-censorship.

The important question is whether perceptions of freedom and political tolerance are related even after controlling for their common origins in the personality traits of individuals. One fairly conservative test of the hypothesis involves calculating semipartial coefficients for tolerance as a predictor of perceived freedom and for perceived freedom as a predictor of tolerance (in both cases controlling for the personality traits). Among whites, this test leads to the conclusion that tolerance and perceptions of freedom are still connected, even with personality attributes controlled. Perceptions of freedom are related to tolerance, and tolerance is related to perceptions of governmental constraints and behavioral self-censorship but not to reluctance to discuss politics. Among blacks, the relationships are similar but generally weaker. Thus, even with a quite demanding test, there is evidence that tolerance and freedom are connected at the individual level. This is compatible with the hypothesis that intolerance may serve as an important constraint on freedom.

21. For an excellent example of this sort of research design see the series of papers by Huckfeldt and Sprague (e.g. 1988, 1989, 1991).

22. For the approach to discussion networks used in the GSS, see Burt 1984 and Marsden 1987.

23. Networks differ, of course, in a variety of aspects, including size, density, homogeneity, dispersion, span, reachability, and anchorage (and, of course, individual ties within the network can be characterized in a number of ways). By design, some of these attributes do not vary within this data set (e.g., size). From a theoretical point of view, peer group diversity is the most interesting attribute for political tolerance and freedom.

24. It is unclear whether respondents can accurately report the party identifications of their friends. For my purposes, this is not of great concern because it is the respondents' *perceptions* of their friends' affiliations that is crucial, not the true affiliations. Moreover, if there is bias in the reports, it is probably bias that reduces the measure of political diversity of the peer group and so works against the hypothesis under consideration.

25. This concept of network diversity is derived from Burt's (1983) definition of *network range*. This is simply the degree of diversity within the network (see Marsden 1987). The obverse of a broad range is a *dense network*. As Marsden asserts, density "also measures the potential strength of normative pressures toward conformity by indicating the capacity of alters to collectively influence the respondent" (p. 124).

26. Some measures of network diversity consider only the diversity of the network work members without regard to the characteristics of the primary respondent (e.g., Marsden 1987). Since I am specifically concerned with partisan diversity and its impact on freedom and conformity, it is important for my purposes that each dyad be scored for diversity or similarity.

27. This measure is based on six-point frequency-of-discussion variables for each of the three peers. Discussion with a peer of a differing party identification is weighted at 1.00; discussion with a peer of the same identification is discounted by .33; and discussion with a peer of unknown identification is discounted by .67. The overall index of diversity is simply the sum of the three discussion diversity measures (i.e., the measure for each peer). This measure ranges from a minimum of 1 to a high of 18 (mean = 6.8; standard deviation = 3.4). Racial differences are statistically significant, with whites reporting slightly greater peer group diversity than blacks.

28. It should be noted, however, that there are weak but nonzero relationships between the ability to report the party identifications of the peer group and levels of perceived freedom. Those who are more knowledgeable about their friends tend to perceive greater political freedom.

29. Marsden (1987) has shown that network diversity varies according to age (range is greatest among the young and middle-aged), education (more education is associated with greater diversity), race (whites have more diverse networks), and urban residence. In order to guard against the possibility that the findings reported in Table 3 are spurious, I estimated each equation separately by race, controlling for the respondent's age, education, and size of place of residence. The resulting regression coefficients are slightly smaller than those shown in Table 3, but the impact of heterogeneity–politicization on whites' perception of governmental constraints is still highly significant, as is the coefficient for heterogeneity–politicization on blacks' behavioral self-censorship.

30. The most systematic assessment of the reliability of spouse reports is Niemi 1974. Niemi concludes that spouse reports on the sorts of attributes considered here are extremely accurate (see also Weiner 1978). In a somewhat different context, Williams and Thomson have concluded, "Systematic errors in proxy reports are not a major problem. . . . Had we relied only on self and proxy reports collected from wives, our pattern of results would have been about the same as those obtained by using data collected from both spouses, but our estimates would have been attenuated by random measurement error" (1985, 120). Brickell, Huckfeldt, and Sprague (1988) have also shown that spouses are extremely accurate in reporting their mate's political preferences.

31. This technique closely parallels one employed by Huckfeldt and Sprague (1991). Based on their South Bend, Indiana, sample, they estimated a Reagan vote probability for each person with whom the primary respondent reported discussing politics. The equation was estimated from data on the primary respondent, regressing (logistic regression) vote choice on seven demographic attributes (e.g., union membership, education, income, age). The resulting equation was then used to estimate a *discussant* probability, based on the scores of the discussant on the seven demographic attributes. Except for the fact that the discussant attributes were gathered through direct interviews with the discussants, this technique is identical to the one I have employed here. For the specific details of the technique they use, see Huckfeldt and Sprague 1991, p. 132, n. 2.

32. The dependent variable is an index based on the responses to 15 tolerance items in the spring survey. The tolerance items involve three activities and five groups. The spring responses were used instead of the summer responses because (1) there are more respondents in the spring due to the ineligibility and nonresponse of some respondents in the second-wave interviews and (2) the measures of intolerance are temporally separate from the measures of perceptions of

freedom. The effect of the latter consideration is to *reduce* the size of the observed correlations.

33. Note that family income is a variable shared by the respondent and the spouse.

34. This technique is not unlike that developed by Charles Franklin (1989) for estimating missing data across data sets.

35. Note that the independent and dependent variables do not "fit" perfectly. The independent variable characterizes the tolerance of the respondent's spouse, while the dependent variable refers to familial discussions in general. This mismatch can only serve to weaken the observed relationships and thus works against the hypothesis.

36. Note that Huckfeldt and Sprague discovered an especially strong effect of the spouse's vote probability on the main respondent's vote (1991, 133). The effect of nonrelative discussants is quite a bit smaller and does not achieve statistical significance in their model.

37. Men and women differ little in their self-described levels of interest in politics. Gender is not a good predictor of interest (tau-beta = −.11), although about 10% more men than women claim high levels of political interest. Nor are there gender-based differences in levels of political knowledge.

38. The total set of tolerance questions were asked in 1976, 1977, 1980, 1982, 1984, 1985, 1987, 1988, 1989, and 1990. Roughly fourteen thousand respondents answered these questions.

39. The critique of these measures is that there is no assurance that the respondents dislike the groups and that without antipathy, tolerance is not relevant. Though it is presumed that most Americans do not care for communists, homosexuals, etc., no independent measure of affect toward these groups has been available. Without some assurance that these groups present a negative stimulus to most respondents, the conceptual requirement that one cannot *tolerate* that which one does not dislike cannot be met. Affect toward these and a variety of other groups was measured in my 1987 survey using a scale ranging from 1 (*like very much*) to 11 (*dislike very much*). The results were reported in Gibson 1989b. These data reveal that while there is no unanimity that these groups are highly disliked, there is a tremendous amount of agreement on the matter; and for great majorities of the respondents, the groups are a valid tolerance stimulus.

40. Each item was scored as 0 (intolerant), 1 (tolerant) and .5 (uncertain).

41. Some additional blocks are associated with the black oversamples that NORC has employed during the 1980s. These bring the total number of blocks to 612.

42. For the primary sampling units represented in the 1987 survey (i.e., excluding the special black primary sampling units that were not used in 1987), the averaged, weighted numbers of respondents per unit is 92, with a standard deviation of 32 and with a minimum of 55, and a maximum of 269, respondents. The average number of respondents per block is 13, with a standard deviation of 4.8, and a range of weighted Ns from 1 to 40. There are a total of 281 blocks represented in the data file for 1987. Note that this analysis is based only on the data from the surveys using the common 1980s sampling frame.

43. At the level of the individual, the correlation between the two tolerance measures is .63 for whites and .38 for blacks. Note that the Stouffer-based measure was derived from questions asked in the spring 1987 survey, while the generalized tolerance measure was constructed from summer 1987 questions.

44. Jennings (1987) has shown just how powerful the effect of dissent can be. Those who engaged in protest during the Vietnam War era are even today more likely to support civil liberties than those who did not protest, despite the fact that protesters and nonprotesters differed little in their attitudes prior to the college experience. Both groups became more supportive of civil liberties as a result of their college experience, but the experience of having participated in a demonstration or sit-in had a distinctive and lasting effect. To the extent that willingness to engage in protest behavior diminishes, support for the broad exercise of civil liberties will decline. See also Jennings and Niemi 1981, chap. 11.

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BENJAMIN FRANKLIN'S *AUTOBIOGRAPHY* AND THE EDUCATION OF AMERICA

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Franklin's Autobiography *was written in part to provide a model for the emerging democratic individual and democratic culture of America. Franklin's teaching in the work has been subject to severe criticisms from the beginning, though it has had many defenders, too. Neither friend nor foe, however, has taken a sustained look at the* Autobiography *itself to explore its teaching in detail. I look at Franklin's presentation of the relationship of wealth and virtue, his utilitarianism, and his vision of democratic society and find a subtle and robust ideal deftly calculated to educate and elevate American culture.*

Benjamin Franklin's *Autobiography* has lived a curious life. Begun by Franklin as a memoir to his son and his further posterity, as well as a vehicle for the pleasure found in recollection (1964, 43), it quickly took on—if it did not have from the start—a more public purpose (see Dawson 1977/78; Lerner 1987, chap. 1; Seavey 1988). Franklin inserted two letters at the beginning of part 2 to confirm that the *Autobiography*'s serious mission was nothing less than the education of a new nation, using Franklin's life and mind as its model. In fact, the work became not only a staple of American popular culture but a conduit through which that culture passed to other parts of the globe. It was printed and reprinted, in the United States and Europe, through the nineteenth century and beyond. It was influential in Japan at a time when the modern Japanese character was forming (Ki 1987). Parts of it made their way into McGuffy's readers, and Parson Weems did it the dubious honor of producing a bowdlerized version (Franklin 1964, 9–12).[1] Yet the work, like the man himself, has also had fierce critics; and these seem to have got the upper hand in this century, when Franklin's work—indeed, his reputation as a whole—are in profound eclipse. Of course, all the figures and ideas of the founding period are in relative eclipse now, with philosophy, politics, and even constitutional law sometimes claiming to have superseded the eighteenth-century models and popular culture's memory stretching barely two decades back. Still, Franklin's fall, from such a height, has been particularly stunning.

Some of the reaction against Franklin and his *Autobiography* stems from the view that they represent a quasi-Puritan outlook, or the outlook of Puritanism's alleged stepchild, the "Protestant ethic." Max Weber, who first defined this ethic, argued that Franklin's *Autobiography* embodies its "ideal type," in all its grim dutifulness (1958, esp. 48–50).[2] In a similar vein, Labaree notes that Mark Twain half-humorously inveighed against Franklin for producing an impossible deal of industriousness to be thrown up to all boys by their parents (Franklin 1964, 14–15). But if some critics have thought Franklin too strict, another strain of criticism finds him and his *Autobiography* too lax, in a moral sense. In his own day, Abigail Adams thought Franklin almost a libertine. John Adams criticized him for being all appearance, devoid of real substance (though Adams seems to have thought this about most everyone except himself).[3] This criticism, along with the notion that Franklin was morally shallow and self-serving, was carried forward by Adams's posterity and latter-day political sympathizers.[4] In the twentieth century, Franklin has been attacked to much greater effect by D. H. Lawrence for being morally base: materialistic, bourgeois, calculating.[5] And most recently, Alisdair MacIntyre, in his attempt to resuscitate a morality based on the virtues, has dismissed Franklin's moral ideal as shallow and utilitarian (MacIntyre 1984, 198–99, 243).

My purpose here is to reopen the question of Franklin's moral teaching and outlook as he presents it in his *Autobiography*; for while Franklin has had no lack of defenders, surprisingly little close attention has been paid to the text of his *Autobiography* with a view to drawing out the precise character of the moral ideal Franklin is promulgating through it. Such attention reveals a surprisingly subtle and multilayered moral teaching, one that easily escapes most of the criticisms that have been leveled against it. Its presentation is also carefully calibrated to influence the widest and most varied American audience. Education (including moral education) was a matter Franklin took seriously all his life, as many of his writings and projects attest.[6] The *Autobiography*, as it stands, is the final and most comprehensive monument of that concern, undertaking, as it does, to help shape the emerging American character.[7] Despite its casual and almost random appearance, the work contains a mature and consistent outlook on morality and the well-lived life and even a distinctive strategy for persuading its readers of the merits of that outlook.[8]

I do not propose to argue the elevation of Franklin's moral ideal when matched against such competitors as MacIntyre's Aquinas or its purity compared to Parson Weems's puritanism. I do propose, however, that Franklin's *Autobiography* fulfills much better than either of these the purpose of providing a new nation and a newly emerging culture with a model that is appropriate to them, one that makes a bid to elevate them in a manner and to a degree that they are most likely to benefit from. The Franklin of the *Autobiography* is neither a classical hero nor a saint; but he does embody an ideal of a distinctly modern and democratic sort. In fact, those parts of the *Autobiography*'s teaching that are most decried as vulgar or materialistic stem from Franklin's deep egalitarianism.[9] He gives scope to the American impulse for material advancement (which it would not be possible or *fair* to suppress); but he gives that impulse a push in the direction of virtue and moderation, understood to be sure in a distinctive way. Equally important, he gives Americans an ideal of democratic public service that is quite high-minded, without being beyond the grasp of ordinary citizens. Franklin goes so far as to downplay in the *Autobiography* elements of his own history that are beyond the reach of ordinary human beings. At the same time, the *Autobiography* can inspire those who are drawn to the higher stature of Franklin himself, giving them important lessons on the place of a man of superior ability and accomplishment in a democratic milieu.

WEALTH AND VIRTUE

One of the leading themes of the *Autobiography* and one that Franklin takes evident pleasure in emphasizing is his own astonishing rise from poverty and obscurity to means and celebrity. One of the stated purposes of the work is to accentuate the contrast between his humble beginnings and his later success and to show the means by which it was accomplished (1964, 43). Franklin invites us to contemplate the contrast between his first, bedraggled appearance in Philadelphia and the prosperity and reputation he has since achieved there (p. 75). His first elevation to political office occasions similar reflections (p. 197). It is partly the prominence of this theme that makes the *Autobiography*'s emphasis on worldly success so unmistakable. Permissiveness toward the impulse to material advancement is indeed one of the central—as well as one of the obvious—lessons of the *Autobiography*. It is also one of the most misunderstood.

The passage on the "Art of Virtue" in part 2 (1964, 148–60), which is probably the most famous in the *Autobiography*, is typical. Franklin proposes a list of 13 virtues, along with a method (the Art of Virtue) designed to allow anyone to become proficient in them. As an incentive to follow this method, Franklin portrays the virtues as instrumental, as *useful*. He assures his readers that his own success and happiness in life are owing to their cultivation (p. 157; see also p. 119); he even offers a fundamentally utilitarian general theory of virtue: "Vicious Actions are not hurtful because they are forbidden, but forbidden because they are hurtful, the Nature of Man alone consider'd"; and it is "therefore every one's Interest to be virtuous" (p. 158; see also p. 54). One of the first episodes recorded in the *Autobiography* taught Franklin the lesson "that nothing was useful which was not honest" (p. 54). Franklin's Poor Richard proverbs (which do not form part of the *Autobiography* but are faithful to part of its message) are even more closely associated with this kind of utilitarianism; the compendium of these proverbs that Franklin composed in 1757 is still widely known as *The Way to Wealth*.[10]

This is the side of Franklin that sent D. H. Lawrence into paroxysms of rage and contempt. Lawrence accused Franklin of deforming generations of readers with a stifling moral vision that has no place for true benevolence or nonmercenary virtue, and of turning man into a mechanism by his "method" of self-perfection (Lawrence 1964, chap. 4). We should not, however, let Lawrence's attack color our own reaction to Franklin's utilitarianism; nor should we mistake *The Way to Wealth* as exhaustive of Franklin's moral vision.[11] Franklin had rhetorical reasons for slanting the wisdom of Poor Richard a certain way, as he is careful to tell us in the *Autobiography*. The "Proverbial Sentences" of Poor Richard, he there says, were in general directed toward the humblest audience, the "common People, who bought scarce any other Books" (1964, 164). These proverbs were "chiefly such as inculcated Industry and Frugality, as the Means of procuring Wealth and thereby securing Virtue, it being more difficult for a Man in want to act always honestly, as (to use here one of those Proverbs) *it is hard for an empty Sack to stand upright*" (ibid.).

Franklin is never shy about using money as an incentive to virtue, by insisting that virtue is the surest path to wealth. Precepts of this type are almost as common in the *Autobiography* as in Poor Richard, and it is these that have caused some to paint Franklin as an unregenerate materialist. In fact, though, they reflect only a recognition on Franklin's part of both the inevitable desire for wealth in people who have little and, just as importantly, the poor prospects for virtue in people who have nothing. It is a simple fact that a certain amount of prosperity is a necessary precondition of virtue in a democratic society like America. And Franklin recognizes that material advancement will inevitably preoccupy most of his audience.[12] Tocqueville wrote some fifty years later that effective moral teaching in the new, democratic age would have to reconcile itself, to some degree, to the material and self-centered preoccupations of the majority;[13] and Franklin is not blind to this necessity. He is indeed guilty (as his critics claim) of refusing to regard this necessity as a moral failing. Moral education as envisaged by Franklin never attempts to persuade anyone that the ambition for material advancement should be sacrificed on the alter of virtue. In the American context, (as Tocqueville also implied), such a teaching would be both elitist and ineffectual. Franklin seeks, rather, to begin by directing material ambition to virtue as its means and then as its end, as well. The ultimate purpose of Poor Richard's exhortations, as Franklin says, was not to produce a full sack but one that will stand upright (see Lerner 1987, 52; Stourzh 1954, 15–16).

The notion that virtue is in one's interest in a narrow, economic sense represents only the first level of the *Autobiography*'s moral teaching. As the work's reference to the wisdom of poor Richard implies, the *Autobiography* seeks to address a wider audience—and deliver a morally less elementary message—than Poor Richard's. This is seen clearly enough in the way the work develops its most materialistic themes. Frugality and industry, key virtues in both Poor Richard and the *Autobiography*, are fairly narrowly connected to money-making; and Franklin recommends them highly as such. But he also is careful to subordinate them to both higher goods and other virtues that economic well-being paves the way to. He defines frugality as making no expense "but to do good to others or yourself" (1964, 149), and he justifies his inclusion of it and industry in his list of virtues by noting that the wealth they accumulate makes possible greater sincerity and justice (p. 151). Virtue may be "the means of procuring wealth" (as the *Autobiography*'s synopsis of Poor Richard has it), but wealth serves the purpose of "securing Virtue" in turn (p. 159). In his own case, Franklin attributes to frugality and industry the "Acquisition of his Fortune, with all that Knowledge which enabled him to be an useful Citizen, and obtain'd for him some Degree of Reputation among the Learned" (p. 157).

If Franklin makes the virtues (especially frugality and industry) instrumental to the accumulation of wealth, the latter is firmly subordinated to the virtues in turn and to more noble pursuits, including, in Franklin's own case, philosophy or science. The *Autobiography*'s elementary and, we might say, propaedeutic, utilitarianism should not conceal the fact that the work as a whole is actually directed strongly away from the concern for wealth as an end in itself. The example of Franklin's own business career is most eloquent. After prospering as a printer with the aid of his frugality and industry, Franklin retired at the age of 42 with a "sufficient tho' moderate Fortune" to allow him to pursue "Philosophical Studies and Amusements" (1964, 196; see also 119, 125). Like almost every American, Franklin had to concern himself with wealth first; but after his needs were comfortably met (Franklin actually became rather wealthy), he retired from commerce for higher pursuits. Franklin's description of these higher pursuits bears more than a passing resemblance to the classical vision of the life of noble leisure, especially when we add to it the public service that was immediately pressed upon him, which he accepted.

The *Autobiography* first caters to the reader's presumptive (and presumptively legitimate) concern with wealth, then directs it toward a much fuller vision of human happiness and the well-lived life. Public service, a reasonable or tolerant skepticism, and benevolent sociability are the elements of this vision that we shall concern ourselves with. For the present, we should notice that while the *Autobiography* shows us a number of ways in which Franklin learned or changed his perspective over the course of his life, it never shows us a time when his concern was purely commercial or monetary. This is true to fact. In one of his youthful "Busy-Body"essays for example, Franklin praises the virtue of the ancient statesman Cato, then asks, "Who would not rather chuse, if it were in his Choice, to merit the above Character, than be the richest, the most learned, or the most powerful Man in the Province without it?" ([1907] 1970, vol. 2, p. 108)[14] Cato is hardly a bourgeois ideal (the essay emphasizes his poverty); yet Franklin leaves very much open the possibility of acquiring riches, learning, and power while at the same time meriting a good character of reputation. It was always Franklin's notion that the four should be combined, which is possible if virtue (as he understands it) becomes the means, as well as the end, of the other ambitions. The moral outlook that this project embodies might be more earth-bound; but it is also more practicable, and safer, than that proposed by D. H. Lawrence in its place.[15]

THE VIRTUE OF REASONABLENESS

Once we get beyond the simple view of the Franklin of the *Autobiography* as a stern master of bourgeois discipline or a mercenary and try to explore more seriously his view of the place of virtue in a well-lived life, we are met with an entirely different set of puzzles. Despite the fact that the *Autobiography* tries to inspire its readers with a sincere devotion to virtue, Franklin's own devotion as portrayed at points in the work has to be described as casual at best. A gentle irony suffuses his account of the art of virtue, which began as a youthful "Project of achieving moral perfection" (see Sayre 1963, 522). "I knew, or thought I knew, what was right and wrong" (1964, 148), Franklin says at the outset of this account; but even with this unguarded presumption the project was not a complete success. Franklin confesses that by the end, he gave up trying to perfect the one virtue he found most difficult, namely, order. It was not worth the effort, given his natural disinclination to it (pp. 155–56). Similarly, perfect humility has escaped him (p. 160). Added to this is the fact that the virtues themselves (as Franklin identifies them) are hardly models of moral strictness. Some of them, like silence, order, and cleanliness—not to mention industry and frugality—would scarcely have merited mention by most earlier moralists (see Pangle 1988, 297–98). These might be thought to belong to a Puritan tradition; but then, there is Franklin's definition of *chastity*: "Rarely [to] use Venery but for Health or Offspring; Never to Dulness, Weakness, or the Injury of your own or another's Peace or Reputation" (p. 150). We learn enough in the *Autobiography* to realize that each of the loopholes in this definition is deliberate; if Franklin had viewed the virtue more strictly, he might have been forced to give up on it, as he did on order. His youthful "Intrigues with low Women" are by and large excused by him as products of necessity ("that hard-to-be-govern'd Passion of Youth," p. 128; see also p. 115). He does *not* include them among the "Errata" he occasionally atones for in the *Autobiography*.

This remarkable indulgence can be explained only partially by the fact that the work itself is addressed to a son who may have been a product of one of these intrigues. It can also be explained only partially by the fact that the art of virtue is designed to put a level of virtue within the grasp of every man and that like many of the sayings of Poor Richard, it thus represents the beginning, rather than the end, of a full human life (see Meyer 1987, 158). The fact is that Franklin's moral teaching as a whole is conspicuously more tolerant than that of many of his religious or philosophical forebears. In fact, it is one of the principal, if less explicit, purposes of the *Autobiography* to *combat* the kind of moralism that had been characteristic of earlier traditions, particularly those of Puritan America. The moral code Franklin wants to replace them with, however, is somewhat elusive.

One way to get at Franklin's own moral outlook is by asking what in his view justifies a new moral tolerance. In the *Autobiography*, this tolerance is grounded partially in a distinctive notion of *reasonableness*. Franklin's reasonableness is different from the rationalism typical of the eighteenth century. Eighteenth-century rationalism sought answers to scientific, moral, and religious questions by means of unassisted human reason; and Franklin does likewise. But governing the operation of his rationalism is a pragmatic and skeptical *bon sens*, which, among other things, acts as a brake on speculative reason itself. This "reasonableness" is prior to morality and even to a religion, in the sense that it governs and moderates them. It is the resolute enemy of intolerance and zealotry, even the zealotry of reason. It is this trait, as much as wealth *or* virtue, that accounts for Franklin's happiness and the pleasing character of his example. In the course of a rather turbulent life, Franklin remains the picture of equanimity, kept always on an even keel by his earthy reasonableness, an amiable trait that is infectious—and is meant to be.

According to the *Autobiography*, this trait was not native to Franklin. His early inclination was, rather, to dogmatism. Upon reading a tract at age 16 equating flesh eating with murder, he became a principled vegetarian (1964, 63). This lasted until he was sorely tempted by some cod during his first voyage to Philadelphia. "I balanc'd some time between Principle and Inclination," Franklin says, then reasoned that since the cod were seen to have smaller fish in their stomachs, he could eat them in turn (p. 87). He concludes, "So convenient a thing it is to be a *reasonable Creature*, since it enables one to find or make a Reason for every thing one has a mind to do" (p. 88). The same sentiment appears more elaborately in his account of the art of virtue, to justify Franklin's abandonment of the goal of achieving moral perfection: "For something that pretended to be Reason was every now and then suggesting to me, that such extream Nicety as I exacted of my self might be a kind of Foppery in Morals, which if it were known would make me ridiculous; that a perfect Character might be attended with the Inconvenience of being envied and hated; and that a benevolent Man should allow a few Faults in himself, to keep his Friends in Countenance" (p. 156). The obvious playfulness of these two passages does not undercut the principle of reason or reasonableness that they outline, a principle that Franklin clearly acted on in both cases.[16] In typically satiric and self-deprecating fashion, these passages point to a view of the moderating role of reason in moral deliberation that is central to the outlook of the *Autobiography*.

Some of the most revealing applications of this view are seen in Franklin's treatment of religion. Here, Franklin's early dogmatism was at its strongest. Born into a pious Boston household, from an early age he devoured writings of Plutarch, Defoe, and Mather, along with books of "polemic Divinity" that he found in his father's library (1964, 58). The mature Franklin regrets only the last: their argumentative style, he says, made him overly disputatious (pp. 58, 60). The arguments themselves, however, had quite a different effect, causing him to doubt revelation by age 15 (p. 113). Soon after, books written against Deism turned him into "a thorough Deist" (p. 114); but this only fed his taste for speculative religious and moral dispute. While working in London at age 19, he composed a pamphlet called *A Dissertation on Liberty and Necessity, Pleasure and Pain* ([1907] 1970, 57–71), which purported to prove that there is no free will, that all our actions are determined by pleasure and pain, and that evil does not exist.

Franklin's London employer thought this production abominable, and the mature Franklin concurs (1964, 96, 114; see also p. 74). In the *Autobiography*, Franklin mentions the work only to disown it and to make a point about what he learned from it. He says he abandoned its doctrine when he discovered that it "perverted" some of the friends he imparted it to, who "afterwards wrong'd me greatly without the least Compunction" (p. 114; see also pp. 89–90). He concluded that "this Doctrine tho' it might be true, was not very useful" and at the same time "grew convinc'd that *Truth, Sincerity and Integrity* in Dealings between Man and Man"—qualities undermined by the doctrine—are necessary to the felicity of life (ibid.). Guided by this new insight, he gave up his speculative creed for something new.

The lesson we are supposed to draw from this episode is complex. Franklin's abandoning Deism on the grounds of disutility "tho' it might be true" is not as misologist as it sounds; for he immediately adds that he suspected some error had crept into his reasoning, making it neither true *nor* useful. But Franklin claims that such errors and their effects are "common in metaphysical Reasonings" (1964, 114), which constitutes an indictment of at least a certain type or application of reason. In a letter to Benjamin Vaughan of November 9, 1779, Franklin confided that the *Dissertation*, together with a later essay maintaining more or less the opposite thesis, caused him to abandon this type of speculation altogether: "The great uncertainty I found in metaphysical reasonings disgusted me, and I quitted that kind of reading and study for others more satisfactory" [1907] 1970, vol. 6, pp. 410–13, esp. 412). The abandonment of speculative or metaphysical reason, however, is not the abandonment of reason per se. As the *Autobiography* and his entire oeuvre make clear, Franklin remains a dedicated rationalist. But his rationalism is informed by a strong sense of the limits of pure speculation.

Franklin's mature attitude is captured in his well-known address at the conclusion of the Federal Convention of 1787 in Philadelphia: "I confess, that I do not entirely approve of this Constitution at present; but, Sir, I am not sure I shall never approve it; for, having lived long, I have experienced many instances of being obliged, by better information or fuller consideration, to change my opinions even on important subjects, which I once thought right, but found to be otherwise. It is therefore that, the older I grow, the more apt I am to doubt my own judgment of others." ([1907] 1970, vol. 9, pp. 607–9).[17] The Convention had failed to adopt any of his major proposals (Becker 1931, 595);[18] but by the end, Franklin held that the practical matter of administering the government well was more important than the grand theories of government bandied about the convention ([1907] 1970, vol. 9, pp. 608–9).[19] This does not mean that he despairs of rational guidance in matters of government, only that such guidance will be halting and must constantly be tested against experience.

One of the key lessons of the *Autobiography* concerns the fallibility of human reason when pushed into the realm of pure speculation; but Franklin does not push this too far, either. Readers of the work gain a sense of the incompleteness of our knowledge but in a way that leads to moderation, rather than debilitating skepticism. What Franklin calls *Reasonableness* serves in the *Autobiography* to block the implementation of speculative conclusions in philosophy or theology; but it does so in the name of Franklin's own more earthy and commonsensical understanding of the proper way of life. It is in this spirit that the work launches a gentle but pervasive and insistent assault on religious dogmatism. The *Autobiography* is unquestionably at its most satirical in portraying dogmatic sectarians. Most unforgettable is Samuel Keimer, Franklin's first employer in Philadelphia. Keimer, a man of unconventional (not to say ludicrous) religious beliefs, was so impressed with young Ben's abilities in debate that he proposed to establish a new religious sect, with himself in the place of the prophet and Franklin as the confuter of opponents (1964, 88). Franklin agreed, on condition of making vegetarianism one of the tenets of the sect. For Keimer "was usually a great Glutton, and I promis'd my self some Diversion in half-starving him" (pp. 88–89). This diversion lasted three months, until Keimer, lusting after the "Flesh Pots of Egypt," gave in and devoured an entire roast pig (p. 89).

The humorous juxtaposition of Keimer's religious dogmatism and vegetarianism is an appropriate one, inasmuch as both are associated in the *Autobiography* with a failure of reasonableness in Franklin's sense. There is more impatience than humor, meanwhile, in Franklin's presentation of the Quakers' doctrinaire pacifism. Franklin illustrates how it forced the Quakers into hypocritical subterfuges and disingenuous evasions when faced with the necessity of providing for the common defense (1964, 186–90). As he dryly comments in a parallel context, "Common Sense aided by present Danger, will sometimes be too strong for whimsicall Opinions" (p. 232). The actions of the Quakers, as opposed to their doctrine, testify to the greater power, as well as sagacity, of common sense and practical necessity. At other points in the *Autobiography*, Franklin's response to strong religious devotion is simply to bury it in silence. He conspicuously refuses to be moved by the piety of a devout Catholic woman he encounters in London (p. 103; see Lerner 1987, 57), by the renowned preacher George Whitefield when he visits Philadelphia, or by the whole religious revival of the Great Awakening (pp. 175–80). Readers of the *Autobiography* subtly imbibe a cool and reasonable attitude toward religious matters from Franklin himself.

This does not mean that Franklin is pushing his readers to irreligion, however; for another of the key lessons of the *Autobiography* is that religion can and should be reasonable. Franklin pointedly approves of the Dunkers, a sect that refused to publish its doctrines, on the grounds that they might later have to be changed (1964, 190–91). "This Modesty in a Sect is perhaps a singular Instance in the History of Mankind," he marvels, "every other Sect supposing itself in Possession of all Truth" (p. 191). The modest admission of fallibility, together with the Dunkers' openness to progress, brings them closest to what Franklin seeks in religion. He assures us that he himself never doubted the existence of God (p. 146)[20] and recommends a creed that could be classified as a species of Deism, with a providential God and a strong emphasis on good works. He lists its tenets as follows:

> That there is one God who made all things.
> That he governs the World by his Providence.
> That he ought to be worshipped by Adoration, Prayer and Thanksgiving.
> But that the most acceptable Service of God is doing Good to Man.
> That the soul is immortal.
> And that God will certainly reward Virtue and punish Vice either here or hereafter. (p. 162)

Franklin takes the attitude that any principles beyond these are purely speculative and refuses to get embroiled in what he necessarily regards as fruitless controversy. This does not lead him to intolerance; rather, he considers his list to contain the "Essentials of every known Religion." He insists he respects every religious belief he has encountered in America (except Keimer's?) and has indeed supported all that came to him for assistance (pp. 146–147).

Franklin's religious tolerance shares some of the characteristic paradoxes of American toleration. His own generic creed paves the way to maximum toleration; but it can have this effect only to the extent that others are brought, like him, to see all doctrines not in it as "nonessential." According to the creed, the divinity of Christ is nonessential,[21] as are myriad doctrines less fundamental but more controversial in the American context. Moreover, Franklin's toleration is qualified by the social thrust of his own religious concern. He finds that individual sects are worthy of more or less respect, depending upon how well they support morality and social concord (1964, 146). Some doctrines might obviously detract from these, but Franklin has little patience even with those that do not contribute adequately to them. He tells us that when he realized that the minister of his own congregation desired "rather to make us Presbyterians than good Citizens," in disgust he stopped attending his services (p. 147).

What Franklin says about his experience with Deism seems to contain the key to his attitude. We recall that that experience taught him the overriding importance of "*Truth, Sincerity and Integrity* in Dealings between Man and Man." This ideal is fully visible in his own creed, and it is by this standard that he now judges at least the extraneous or supernumerary dogmas of all sects. As a result, Franklin's approach to religion is primarily social and moral—indeed, this-worldly rather than otherwordly. It is "reasonable" in the sense of taking its bearing by experience, by what is visible; and it brackets, as much as possible, all purely theological questions. In effect, he subjects religion to a test of social utility. Whether Franklin is an unalloyed utilitarian in religion is a subtle question, depending upon whether even his core religious principles are derived simply from his conviction that they are useful to society.

This question cannot be answered with certainty here—perhaps not anywhere. It does, however, allow us to understand something more about the important question of Franklin's utilitarianism. When explaining his attitude toward biblical revelation, for example, Franklin has the following to say: "I entertain'd an Opinion, that tho' certain Actions might not be bad *because* they were forbidden by it, or good *because* it commanded them; yet probably those Actions might be forbidden *because* they were bad for us, or commanded *because* they were beneficial to us, in their own Natures, all the Circumstances of things considered" (1964, 115). Thus, the Gospels' moral strictures are considered valid not as revealed truth but as principles of utility. And although Franklin specifically denies that his own religious principles are derived from revelation (pp. 114–15), he does apply the same kind of calculus to morals in general. Speaking of the treatise he intended to write on the art of virtue, he says, "It was my Design to explain and enforce this Doctrine, that vicious Actions are not hurtful because they are forbidden, but forbidden because they are hurtful, the Nature of Man alone consider'd: That it was therefore every one's Interest to be virtuous, who wish'd to be happy even in this World" (p. 158). These are the classic statements of Franklin's moral utilitarianism. Since they reduce morality and religion quite clearly to self-interest, they make Franklin appear to some to be nothing but a vulgar mercenary. But Franklin's argument harbors some subtleties that need to be noticed. Utilitarianism can take many forms; and the elevation or debasement of each depends upon the goods, or the vision of the human good, that it rests upon or is directed toward. Things can be judged "useful" or the reverse only with reference to these goods, considered as goals. As the second of these passages points out, the real issue is the nature of man and what truly conduces to his happiness. In the *Autobiography*, this passage follows a general account of the utility of the virtues to Franklin's own happiness, where he makes clear that for him, happiness includes prosperity and reputation, as well as learning and public service (p. 157). In the first passage, the proviso that moral actions are beneficial to us "all the Circumstances of things considered" seems also to allude to a rich understanding of the human good that underlies utility for Franklin. This passage is part of Franklin's account of his turn away from Deism and his adoption of the view that "*Truth, Sincerity and Integrity* in Dealings between Man and Man" are necessary to the felicity of life (p. 114). It is with reference to this ideal that religion, as well as virtue, are recommended as "useful" by Franklin in the *Autobiography*. In addition to its richness, his ideal fully connects the individual and the social good. But it is only adumbrated in these two passages; its full articulation is seen only in the portrait of Franklin's life as a whole in the *Autobiography*.

Utilitarianism, as Franklin envisions it, is anything but mercenary or impoverished, because the goal or purpose that drives it is generous and humane. In fact, Franklin might have found inspiration for it in the Socrates of Xenophon's *Memorabilia*, which he encountered and admired at an early age, and which also makes use of utilitarian logic (Franklin 1964, 64, see also Xenophon *Memorabilia* 3.8, 4.1.1, 4.2.24–36 [1968]). Franklin might have found the inspiration for his reasonableness there, as well; for we may now say that this reasonableness represents a combination of metaphysical agnosticism with strong convictions about certain of the goods of life. The convictions are a product of combined observation and reasoning, and they serve to restrain metaphysical speculation in its excesses. It has been said that Franklin, in the thoroughgoing practicality of his approach to life, sacrificed reason to experience (Stourzh 1953, 1096; idem 1954, 9; see also Seavey 1988, 4). It is more appropriate to say that he concluded, in the face of the limits of speculation, that reason must take a vision of the human good anchored more securely in experience as its first and guiding principle. For this reason Franklin might be considered among what William James called the forerunners or "preluders" of American pragmatism (James 1981, 27). James also identified Socrates as one of these preluders (ibid.). But Franklin would not endorse the development of a new metaphysics out of his approach. More importantly, he clearly regards his convictions or principles to be objective, not radically bound to his own experience. In this respect, he remains closer to the Socratic approach than to that of Pragmatism.

The indulgent or forgiving character of Franklin's moral outlook, at least when measured against those of some of his critics, thus has two sources. The first is his utilitarianism. Every utilitarian approach to morals refuses to treat virtue as an end in itself, and Franklin quite frankly says that virtue and at least part of religion are normative only because they contribute to individual happiness and the social good. Franklin is fully aware of the character of his argument and how it diverges from other perspectives—pious perspectives, in particular. Yet much of the criticism he has faced—that which makes him a mercenary simply because he regards virtue as a means to something else—is rooted in precisely the "deontological" approach to morals he rejects (see Pangle 1988, 18). For Franklin, there is nothing more natural than for there to be a happy coincidence of virtue and utility and nothing more ill conceived than to propose a radical separation of the two.

In addition, Franklin's distinctive vision of how extramoral human goals and appetites fit into a full and happy life makes his utilitarianism more indulgent than others'. For him, wealth, power, and reputation, as well as virtue, are legitimate parts of such a life. Here, the crucial contribution comes from Franklin's own experience of the felicity of life, which has persuaded him that much more of human nature can safely be indulged than earlier moralists had thought. His good-humored concessions to his own lack of orderliness and pride, his view of chastity, and even his rejection of vegetarianism reflect Franklin's assessment of how far moral strictures may be relaxed. This is reflected, as well, in the half-humorous proposal he once penned to devise scientific means of scenting intestinal gas so that flatulence might become socially acceptable ([1907] 1970 952–55). Franklin is at all times in favor of liberating innocuous natural impulses. The suppression of human appetites in the name of transcendent goals unrelated to human happiness simply appears nonsensical—unreasonable—to him. Reasonableness for Franklin consists precisely in affirming appetite where appetite contributes to human happiness and where nothing more than speculative grounds can be given for disturbing it. It is a kind of moderation directed against the excesses of moralism itself.

Franklin's vision of the good life, which lies behind all of this, is the greatest unifying theme of the *Autobiography*. It is a vision that serves not only individual happiness, but the good of society as a whole. But just as Franklin has altered our understanding of virtue to fit his vision of the good life, so he has thought his way to a new and distinctive view of the best social order and thus of the proper nature of sociability. A vision of society that combines in typical fashion benevolence and utility, together with a rather complicated egalitarianism, might be called the capstone of the *Autobiography* as a whole.

THE ART OF SOCIABILITY

If there is one thing that every reader of the *Autobiography* comes to admire and to envy in Franklin, it is his remarkable sociability. The facility with which he makes friends is a constant source of delight to his readers and an evident source of both delight and utility to himself. It is part of Franklin's uniqueness to have been not only universally renowned but universally beloved in his own day.[22] Still more remarkable was his amazing success in mastering all of the milieux he moved in—and these were as varied almost as the world had to offer—without being the less loved for it. From the Boston of his earliest youth, to the more freewheeling business and intellectual world of Philadelphia, to imperial London, and finally to the royal court of France, he displayed an unerring ability to influence events by the friendly force of his character. In all the machinations of his public career, he seems to have made only one serious misreading of men or events.[23] The *Autobiography*, which covers none of Franklin's career as an international diplomatist, devotes considerable attention to his facility with people and aims to communicate something of that facility to us. In typical fashion, Franklin attributes his social success to a number of *techniques* and distills them into what amounts to an art of sociability that may be practiced by anyone. Also in typical fashion, this art looks to both pleasure and utility. The problems it is designed to overcome are partly those encountered by ordinary citizens and partly those confronted by men of superior achievement or ability who aspire to exert leadership in the novel circumstances of egalitarian society.

In the *Autobiography* Franklin portrays his sociability, like his reasonableness, as an acquisition or contrivance, rather than as something native to him. In some ways, the two are sides of a coin. His early encounter with books of "polemic Divinity" led him not only to the intellectual vice of dogmatism but to the social vice of contentiousness—a defect rarely found in men of good sense, Franklin says, "except Lawyers, University Men, and Men of all Sorts that have been bred at Edinborough" (1964, 60). With this turn of mind, all he seems to have gained from his first encounter with Xenophon's Socrates was proficiency in the method of confuting people by innocent-seeming questions (pp. 64–65). He combined mastery of this method with his youthful religious skepticism—to his own delight but the consternation (and alarm) of others. Franklin says that his eristic activity eventually made him notorious enough to be "pointed at with Horror by good People" in Boston (p. 71). He left Boston partly in order to escape this bad reputation and gradually gave up his pugnacious method of dispute, retaining only the Socratic habit of expressing himself less positively in argument, replacing words such as *certainly* or *undoubtedly* with expressions like *I conceive, I apprehend,* or *I imagine* (p. 65).

After his change of manner, Franklin says, his conversations went on much more pleasantly. But this was more than a modification of speaking style. It became also a change in Franklin's whole approach to social relations, away from confrontation and toward accommodation. This new sociability was motivated by the hope of doing good, as well as receiving pleasure. Championing his less assertive mode of speech, Franklin says:

> This Habit I Believe has been of great Advantage to me, when I have had occasion to inculcate my Opinions and persuade Men into Measures that I have been from time to time engag'd in promoting. And as the chief Ends of Conversation are to *inform*, or to be *informed*, to *please* or to *persuade*, I wish wellmeaning sensible Men would not lessen their Power of doing Good by a Positive assuming Manner that seldom fails to disgust, tends to create Opposition, and to defeat every one of those Purposes for which Speech was given to us. (1964, 65)

This remarkable passage not only informs us of the true purposes of speech but gives us implicitly a vision of the proper social order. That order is closely bound up with speech and its purposes. Speech brings pleasure, conveys information, and makes persuasion possible. Potentially a great power of social good, it is too often abused and deflected from its proper use. Franklin's art of sociability therefore rests heavily on a self-conscious reform of men's use of their faculty of speech.

The principal obstacle to proper sociability is pride. Arrogant habits of speech block all its potential for good, disrupting society, as well as the genuine pleasure of conversation. Franklin's own experience has taught him the tenacity of this problem. He admits that when he was developing his art of virtue, it took the prodding of a Quaker friend to get him to add the virtue of humility to his list. He made it his thirteenth virtue and in this context associates it primarily with modesty in speech (1964, 159). He also confesses to the difficulty of acquiring it: humility forces him to forgo the "pleasure" of immediately contradicting others' errors, a discipline that does "some violence to natural Inclination" (ibid.).

Due to this difficulty, Franklin informs us, he has not truly succeeded in acquiring humility, only its appearance. But for social purposes (what concerns Franklin primarily here) this has been perfectly adequate. Under the virtue of humility, Franklin had written "Imitate Jesus and Socrates" (1964, 150); but it seems that in this respect his *imitatio Christi* is confined to externals. One reason why Franklin accepts this with such good humor is that neither his view of individual virtue nor of social relations is one from which all pride or even vanity has been purged. That would require a sterner discipline than Franklin is willing to impose, especially since he finds redeeming elements in human pride, beginning with his own. At the onset of the *Autobiography* he admits ("since my Denial of it will be believ'd by no body") that the work is motivated partly by vanity. But vanity, he says, is something he has learned to indulge, since it "is often productive of Good to the Possessor and to others that are within his Sphere of Action," as well as being one of the "Comforts of Life" (p. 144). As always, the combination of private pleasure and social utility is an irresistable one for Franklin. The problems with pride arise largely because of misconceptions on the part of its possessors. As Franklin says in the early essay in which he lionizes Cato, the major problem with man's "strong natural Desire of being valu'd and esteem'd" is simply that too few know the best way of gratifying it, namely, through virtue ([1907], 1970, vol. 2, p. 108).

The social order as Franklin conceives of it is based more on the proper education of vanity and its mutual accommodation by members of society than on its suppression. Showing vanity how useful virtue is to its gratification (one of the themes of the *Autobiography* as a whole) is a part of its education. Franklin's exhortations to modest speech for the sake of both pleasure and social concord is another part of its education. But since Franklin does not imagine that harmful pride can be so easily brooked, further accommodation is required. Thus, among other things, he admonishes us all, under the heading of "Moderation," "Forbear resenting Injuries so much as you think they deserve" (1964, 150). Pride overvalues slights and overvalues its own desert; and those who are concerned about social accommodation will therefore control their own pride, while giving others' wider berth. This kind of moderation is perfectly captured in a subtle emendation Franklin proposes in the *Autobiography* to a famous poetic couplet. Instead of writing "Immodest Words admit of no Defence; / For Want of Modesty is Want of Sense," (p. 66).[24] Franklin suggests the poet should have written, "Immodest Words admit *but this* Defence, / That Want of Modesty is Want of Sense." Franklin takes the view that immodesty is caused by ignorance and in a spirit of Socratic generosity maintains that ignorance provides some excuse for vice.

Franklin's view of sociability, resting on modesty of speech, cultivation of at least the outward forms of humility, and indulgence of mistaken pride in others, is appropriate, especially in the new kind of society Franklin was attempting to shape. In modern democratic or egalitarian society, as much pride must be indulged in each as civility will bear. Special forbearance will be practiced by those who take the lead in sociability, as Franklin hopes each of his readers will do. But Franklin is the last to think this will be the way with all and is fully aware that those who take such a lead will likely be a minority. The problem of sociability thus opens something of a breech in the smoothly democratic exterior of the *Autobiography*—a breech that becomes wider and more significant, the closer one looks; for out of the art of sociability Franklin develops an art of leadership that is egalitarian in its benevolence toward the public at large but is more exclusive in its audience. In his praise of modest speech, Franklin had put special emphasis on the "Power of doing Good" that modesty would place in the hands of "wellmeaning sensible Men" (1964, 65). These men are like Franklin in his capacity as a projector and a leader—as something other than Everyman. Franklin notes that the humble manner of speech and demeanor he developed was vital not only in smoothing his social relations but in advancing the many projects he undertook during his life and in taking the lead in practical affairs in general (p. 160). It not only brought him good will but allowed him to draw men along by persuasion, without bruising their pride or vanity.

If sociability in a democratic context requires special attention to the universalized claims of pride, the exercise of leadership compounds the difficulties. Leadership, which always requires the careful management of human vanity, is given special delicacy in the democratic milieu as Franklin understands it. Every man demands—and merits—respect; and the leader cannot proceed peremptorily. But Franklin does not take the democratic perspective so exclusively as to think this is the only problem, nor does he think democratic leadership should concern itself only with accommodating the pride of followers. There is also the legitimate pride of the leader. Franklin's own pride is concealed but not conquered; and it stands to reason that the "future great men," for whose benefit, among others, Benjamin Vaughan exhorted Franklin to publish his *Autobiography* (1964, 135), will be at least as proud. In order to put this problem in its proper perspective we must first realize that in predemocratic Western societies (indeed, in the London and Paris of Franklin's experience) such pride was an unquestioned virtue. Under the name of honor, it forms, in fact, the copestone of every aristocratic social order. Franklin is willing, in self-effacing democratic fashion, to call the leader's pride "vanity" and accommodate it to the new social circumstances for which he is writing. But he is not willing to sacrifice it on the alter of egalitarianism. If vanity in its ordinary forms, properly directed, can be productive of much good, the pride that will motivate future Franklins is even more to be cherished. The difficulty is to satisfy both, to give scope to the leader's pride without offending that of his followers.

This cannot be done without a certain amount of subterfuge, and that is one of the things Franklin's art of sociable democratic leadership provides. When seeking supporters for a early project of his, Franklin discovered that resistance was created by the supposition that the project's success would raise his reputation even "the smallest degree" above that of his followers (1964, 143). Here, democratic pride is seen in its ugly form as resentment. Franklin did not, and does not, believe this resentment can simply be done away with, so he developed a ruse for concealing his leadership to get around it. By presenting schemes as the idea of "a number of friends" or of "some *publick-spirited Gentlemen*" who seek wider support, he advises us, opposition rooted in envy or distrust of ambition can be forestalled. He recommends this device to any who would lead effectively (pp. 143, 193). The vanity of followers is assuaged, and the leader's projects go forward more smoothly; but the real beauty of this device, Franklin assures us, is that in the end there is no real sacrifice of the leader's pride: "If it remains a while uncertain to whom the Merit belongs, some one more vain than yourself will be encourag'd to claim it, and then even envy will be dispos'd to do you Justice, by plucking those assum'd Feathers, and restoring them to their right Owner" (p. 143). If Franklin is to be thought of as the apostle of postponed gratification, this might be the best example of it in the *Autobiography*. Precisely by adopting a low profile in deference to the pride of others—by being humble in appearance—the leader garners the praise he wants and deserves. Franklin believes this and devices like it to be necessary to effective leadership in the circumstances he—and we—confront.

The *Autobiography* gives us enough examples of this kind of self-concealment to make it a theme of the work. His first published essays were submitted anonymously to his brother's newspaper, to gain them a fair hearing (1964, 67—68). His long-standing intellectual club, the Junto, was a semisecret organization. Along with ancillary clubs established on the same model, Franklin says the Junto was useful as means of preparing the public mind for projects he later wished to take in hand (pp. 170–71, 173–74). He once formed a plan for a larger group to be called the Society of the Free and Easy, which was to begin as a secret party whose members had trained themselves in his art of virtue and subscribed to his religious creed and who would, in unison, have an impact on public affairs (pp. 161–63). Franklin claims this group was never formed; but he still believes it useful and possible, "as I have always thought that one man of tolerable Abilities may work great Changes, and accomplish great Affairs among Mankind, if he first forms a good Plan" and prosecutes it assiduously to its conclusion (p. 163). This particular plan began with the formation of a secret elite.

The culmination of Franklin's instruction in sociability is an exhortation to public service addressed to everyone; but the most productive kind of public service is carried out by a man who is willing to take the lead while operating within the constraints of the new social setting. Franklin brings a thoroughly democratic sensibility to public service, but he does not blind himself to the fact that the new conditions are constraints. Franklin's leader operates often by stealth, using devices that we would almost call manipulative but for the spirit in which Franklin employs them. The concealments always fall within Franklin's definition of *sincerity*, "[to] Use no hurtful Deceit" (1964, 150). The deceits he recommends always have the purpose of removing obstacles to projects of public benefit. Yet here, unquestionably, the distance between Franklin and his followers—or between Franklin and most of his audience in the *Autobiography*—is greatest. Despite a spirit of infinite benevolence, Franklin's innocently deceitful way of making his way as a leader through the social milieu bespeaks a certain degree of detachment and even of irony toward his fellows. Paradoxically, in the *Autobiography*, as in many of his writings, Franklin holds himself partly in reserve in order to serve the public—and himself—better.

In his *Autobiography*, as in his life, Franklin cultivates a peculiar mixture of intimacy and distance, transparency and concealment. But Franklin's irony, if Socratic in spirit, is not Socratic in extent. Franklin is a democrat and cannot, for all his irony, be portrayed as an esoteric elitist in the classical sense. In most respects he makes himself and his views completely known to his readers (see Morgan 1991). In the *Autobiography* Franklin makes a deliberate effort to portray himself as a model American, imitable by all of his fellow countrymen. His lessons on effective leadership are a partial exception to this. But of all the deceptions or concealments we might find in the *Autobiography*, the most thoroughgoing is Franklin's downplaying of the aspects of his life and activity that put him too far above the average man. This is especially true in the field of intellectual achievement. Though Franklin makes much of his lifelong love of books and of writing, the *Autobiography* uses these things by and large to convey lessons about diligence and the advantages intellectual cultivation might bring to anyone, not to show how Franklin became an internationally renowned thinker and essayist. Similarly, though Franklin mentions his work with electricity, work for which he was already famous when he began the *Autobiography* and that was by and large complete by time of the latest events narrated in the *Autobiography*, it, too, figures in the work mostly as a demonstration of the kinds of virtues the work's wider audience may aspire to. It is one of the noncommercial pursuits that Franklin retires from business for (1964, 196) and provides an instance of his refusing to embroil himself in unnecessary disputes (pp. 243–44). When his scientific inventiveness leads to the development of his "Pennsylvania Fireplace," he emphasizes primarily that he declined the patent on it and on other inventions, out of generosity to mankind (p. 192). In the end, the *Autobiography* gives us a hero who is more ordinary than Franklin himself actually was.

The *Autobiography* breaks off in the year 1757, around the time when Franklin was becoming a national and international statesman, that is, the time when his public career was beginning to take him decisively beyond the aspirations of the ordinary citizen. Franklin did not intend to stop the work there,[25] but this ending point suits very well the didactic purpose of the work as we have it. The Franklin thus bequeathed to posterity is not quite the historical Franklin (as all his biographers know). But the *Autobiography* is not biography in the rigorous sense. It is, rather, an effort to construct a new model American. Franklin was one of the few to embrace modern egalitarian society in its infancy and to try to shape it. And he wishes us to become, as much as possible, like he has made himself—combining the maximum of both virtue and prosperity and contributing to a society graced with both benevolence and charm.

Notes

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1. For general accounts of the history of the work's reputation, see Buxbaum 1987; Miles 1957; Sayre 1963, intro.

2. For the view that Franklin is a Puritan, see Griswold 1965; see also Sanford 1965.

3. See Adams 1955; see also Miles 1957, 118–19, and Miroff 1986, esp. 128. Adams was one of very few to regard even Washington's reputation as unjustified.

4. Miles (1957) speaks of the tradition of Franklin-bashing carried on by what he calls the "Neanderthal wing" of the Federalist party into the nineteenth century. See also Labaree's citations of Charles Francis Adams in Franklin 1964, 14–15.

5. Lawrence wrote two versions of his critique of Franklin, one in 1918 and one in 1923 (Lawrence 1987 and 1964, chap. 4, respectively). The first is more temperate, the second more widely known.

6. See, e.g., the collection *Benjamin Franklin on Education* (Franklin 1962).

7. See the letters Franklin had reprinted at the beginning of part 2 (1964, 133–40); see also the letter from Franklin to Vaughan after he had completed most or all of the *Autobiography* as we have it (Franklin [1907] 1970, vol. 9, pp. 675–77).

8. The four parts of the *Autobiography* were written at three different times in widely differing places (part 1 at Twyford estate, England, 1771; part 2 at Passy, near Paris, 1784; and parts 3 and 4 in Philadelphia at the very end of Franklin's life, 1788 and after). This has led some to find differences of tone, outlook, and intent among the different parts of the work. I believe variations of tone may be found among the parts, but not of outlook or intent.

9. Although Franklin did not commit himself to democracy as a *political* form until rather late (he was a monarchist, though of a radical Whiggish stamp, for most of his life), his egalitarianism was constant. By the time of his participation in the Federal Convention of 1787, he was more populist than most of the members of that body. See Franklin [1907] 1970, 590–604. See also Farrand 1937, vol. 1, pp. 48, 77–78, 81–85; ibid., vol. 2, p. 542; ibid., vol. 3, p. 297; Rossiter 1952; Stourzh 1953, 1107–14.

10. This was first printed as a preface to the *Poor Richard's Almanac* of 1758. It is also known as *Father Abraham's Speech*. See Franklin [1907] 1970, vol. 3, 407–18. Reprintings of this were immensely popular not only in America but in England and on the continent (see Franklin 1964, 164).

11. Most interpreters of Franklin agree that Lawrence took far too narrow a view of Franklin's teaching. See, e.g., Lerner 1988, chap. 1; Miles 1957, 142; Rossiter 1952, 281; Sayre 1963;

Schneider 1955; Seavey 1988. For another critique of Franklin, see Angoff 1955; see also Lemay 1982.

12. Something of Franklin's understanding of his audience can be seen in a pamphlet he wrote in France in February 1784 entitled *Information to Those Who Would Remove to America* ([1907] 1970, 975–83). The pamphlet's message is that prosperity comes surely to those who work in America but that there is no prospect for idlers, placeholders, or courtiers.

13. Tocqueville speaks, for example, of the American doctrine of "self-interest rightly understood" (1969, vol. 2, pt. 2, chap. 8).

14. The essay was originally published in the *American Weekly Mercury* of February 18, 1729, when Franklin was 23. It is true that the image of Cato had been both appropriated and altered by the London literary culture that the young Franklin idolized; but the Cato of this essay is the Cato of Plutarch, not of Trenchard and Gordon. On the "Cato cult" in England and America at this period, see Bailyn 1967, chap. 2; MacDonald 1985, 190–95. On its nonclassical elements, see Pangle 1988, 30–33.

15. The vulgar Nietzscheanism embraced by Lawrence in his critique of Franklin is considerably more dangerous and irresponsible than the sober moderation of Franklin.

16. For yet another instance of this sentiment, see Franklin's letter to Mary Hewson of May 6, 1786, where he excuses his idle cardplaying with the reflection that his immortal soul has enough time lying before it: "So, being easily convinced, and, like other reasonable creatures, satisfied with a small reason, when it is in favour of doing what I have a mind to, I shuffle the cards again, and begin another game" ([1907] 1970, vol. 9, pp. 510–13). This kind of ironic appeal to reason was obviously a favorite with Franklin, but its irony masks a serious reflection.

17. The whole address is a meditation on this theme.

18. Franklin supported proposals for an executive council, an executive without pay, and a unicameral legislature. See Farrand 1937, vol. 1, pp. 48, 77–78, 81–85; ibid., vol. 2, p. 542; ibid., vol. 3, p. 297. He was, however, instrumental in fashioning the compromise on representation by states and by population (vol. 1, pp. 488–89; ibid., vol. 3, pp. 470–73).

19. Alexander Pope, one of Franklin's favorite authors, produced the famous couplet on this theme: "For forms of government let fools contest; / Whate'er is best administered is best" (*Essay on Man* 3.303–4). The succeeding couplet could serve as motto for Franklin's attitude toward religion: "For modes of faith let graceless zealots fight; / His can't be wrong whose life is in the right."

20. Franklin also says he never doubted either that God governs the world with his providence or that the best service to God is service to man. Yet his *Dissertation on Liberty and Necessity, Pleasure and Pain* at least comes very close to denying these things. When speaking about what the notoriously elusive and ironic Franklin actually believed in religious matters, we must be on our guard. Here, especially, we might be mindful of the loophole in his definition of the virtue of sincerity, "Use no hurtful Deceit" (1964, 150).

21. In a letter to Ezra Stiles of March 9, 1790, very near the end of his life, Franklin repeats his religious principles. In response to a query by Stiles, he allows that the "System of Morals" propounded by Jesus is "the best the World ever saw or is likely to see" but that his divinity "is a question I do not dogmatize upon, having never studied it, and think it needless to busy myself with it now, when I expect soon an Opportunity of knowing the Truth with less Trouble" ([1907] 1970, vol. 10, pp. 83–85).

22. Consider the following testimonial—from John Adams, no less: "[His fame] was more universal than that of Leibnitz or Newton, Frederick or Voltaire, and his character more beloved and esteemed than any of them" (quoted in Stourzh 1954, 2).

23. I am thinking of the affair of the Hutchinson letters, which is not covered by the *Autobiography*. Franklin evidently believed that the divulgence of these letters to selected men in Boston would cool passions there in 1772–73, but it actually had the opposite result. He got into serious trouble in London, where he was living as a colonial agent at the time. See Van Doren [1936] 1964, 444–78; Willcox 1987, 105–8. Franklin's course as a youth in Boston was not entirely smooth, which taught him some important lessons. His overall success in his public career, of course, did not preclude his making enemies—as was inevitable, especially in London during the years of revolutionary ferment.

24. Franklin falsely attributes this couplet to Alexander Pope (1964, 66n.). He also misquotes the second line, substituting *Modesty* for the *Decency* of the original.

25. Franklin was working on the *Autobiography* up to the time of his death, and an outline he prepared for it (1964, 267–72) extends chronologically much beyond the point where the work breaks off.

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INTERESTS AND ETHICS IN POLITICS

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I examine the place of self-interest in political life as given by a conception of politics that invokes ethics. This conception portrays each citizen as an individual with unique hopes and desires who is at the same time joined with others—part of, and continually giving shape to, a shared social and political life. It sees in political diversity and controversy not just conflicting interests but also competing claims about what "we"—unique individuals, linked to particular others through social roles and relationships, and together forming a single citizenry—ought to do or seek. Research that simply adopts a broad conception of utility or interest to admit nonselfish preferences or that employs typologies contrasting self-interested with non-self-interested motives will reveal neither the significance nor the limits of self-interest in this politics. Rather, we must explore how citizens' interests are both championed and challenged by the understandings of "good" and "right" to which our politics gives voice.

> Perhaps what characterizes political life is precisely the problem of continually *creating* unity, a public, in a context of diversity, rival claims, unequal power, and conflicting interests.
>
> —Hanna Pitkin, *Wittgenstein and Justice*

Mathematical and empirical scholars of political behavior have frequently subscribed to what James Coleman describes as the "central postulate of economic theory," conceiving of the political actor as *homo oeconomicus*, a "rational man attempting to pursue his selfish interests" (1986, 15). Yet empirical evidence for this self-interest assumption has often been weak, and skeptics of a self-interest-based politics abound (Bellah et al. 1986; Citrin and Green 1990; Cohen 1986; Dagger 1986; Dobel 1986; Elster 1986; Goodin 1990; Grofman and Feld 1988; Kingdon 1988; Mansbridge 1980, 1990a; Reich 1988; Sagoff 1986; Sears and Funk 1990; Sunstein 1990; Tyler, Rasinski, and Griffin 1986). Those rejecting a conception of political actors as self-serving egoists have reached out in two prominent directions.

The first approach retains an assumption that citizens seek to satisfy their preferences, or maximize their utility, or advance their interests, while redefining preferences, utility, or interests. Rational and social choice modelers have commonly rejected the stipulation that preferences reflect only self-regarding or selfish desires by placing no substantive constraints on utility whatsoever: utility can embrace anything that an individual might value (e.g., Arrow 1967; Becker 1976; Downs 1957; Hechter, Opp, and Wippler 1990). As Mancur Olson explains it, "To say that rational decision-making maximizes 'expected utility' says little more than that decision-makers take *all* of their objectives, material and intangible, selfish and altruistic, into account" (1987, 206; emphasis original).[1] Comparable strategies have arisen among scholars who consider the content of "interests": interests need not necessarily be self-regarding but might be other-regarding or principled, as well (e.g., Connolly 1983; Flathman 1966; Held 1970; Mansbridge 1980; Schlozman and Tierney 1986).[2]

The second approach preserves a narrow conception of what constitutes self-interest and then explores the significance of a number of motives each held as conceptually distinct from self-interest. Work in this genre has generated a number of motivational typologies, each contrasting self-regarding or selfish preferences with those reflecting nonselfish concerns: "varieties of unselfishness" (Jencks 1990), duty and love (Mansbridge 1990b), symbolic predispositions (Sears et al 1980), sympathy and commitment (Sen 1990), and justice (Tyler 1990), among others.

Both of these approaches replace a conception of citizens as purely self-regarding or selfish with a more generous view of the ends people might be seeking in politics, while providing different accounts of "self-interest." The first rejects a narrow (and objective) version of self-interest, while retaining the premise that people do seek whatever it is that they find to be in their interest. It conceives of political actors as egoists but not as egotistical. They pursue their interests, but those interests may not be entirely selfish. The second adopts a narrow (and objective) account of self-interest as self-regarding, seeking to understand its unique empirical significance vis-à-vis alternative bases of public opinion and political choices. By confining what it counts as self-interest, it carves out a space for a nonegoist politics.

I offer an alternative account of the place of self-interest in political life, one based upon a conception of politics that invokes ethics. In this politics, I will argue, citizens are not simply seeking to satisfy their personal preferences, be they self-regarding or not; but neither are they simply setting aside their personal preferences or self-regarding interests and adopting some non-self-interested aim. The citizen, instead, must attempt to resolve what Nagel identifies as the central problem of ethics: "how the lives, interests, and welfare of others make claims on us, and how these claims, of various forms, are to be reconciled with the aim of living our own lives"

(1986, 142). When judged against the perspective on citizenship that I will sketch here, simply adopting a broad conception of utility or interest to acknowledge that political preferences might not be purely self-regarding, or employing typologies that contrast self-interested with non-self-interested preferences, are each inadequate as a response to the failures of the economic approach. The first fails to recognize judgments of value that are articulated as public and not personal claims; the second imposes a dichotomy that is either false or incomplete. We limit our understanding of both interests and ethics in politics when we either subsume anything citizens can value under the rubric of personal preferences or focus on adjudicating between rival explanations defined by some conception of a self-interest/altruism divide. Instead, we need to bring conceptions of interests and ethics *together* as we seek to develop a more comprehensive account of public opinion and political choices.

PREFERENCE, JUDGMENT, AND JUSTIFICATION

Bringing ethical ideas to the study of American public opinion and political action means exploring a particular conception of politics and political actors. This politics is not simply an arena where citizen preferences are articulated and aggregated but one where public goals and policies are debated and political choices must be publicly justified.[3] When citizens assess public policies, events, and leaders or consider their own political choices, they are not merely trying to figure out what or who they like or what it is they want. They are also trying to figure out what or who is *good* or what is *right*. The objects of judgment are political, but these questions are ethical.[4] And the answers to ethical questions rest upon justifications.[5]

Ethical justifications may take many forms and may make any number of substantive claims. Consequentialist moral arguments, for example, rest the conclusion of right on a theory of the good: that which is right maximizes good. Utilitarian consequentialists apply a single metric when representing what is good in states of affairs, looking to maximize social utility.[6] Nonutilitarian consequentialists deny the practical intelligibility and moral significance of the reductive utility metric, recognizing multiple standards of value. But such arguments still rest the description "right" on the choice with the best consequences. From nonconsequentialist or deontological perspectives, in contrast, what is right is not what maximizes the good. Instead, some conception of nature, or persons, or rationality, or divine will might be cited as supporting claims about rights, duty, obligation, or justice. Quite apart from the consequences of an action (under any description of consequences), these premises serve to establish a judgment that the action (or practice or policy) is, at least prima facie, right or wrong.

Judgments expressed in the evaluative language of ethics as to what is "good" or "right" lay claim to what *we* should value or what *one* should value, resting upon interpersonal justifications that we can (and often do) disagree about.[7] If, for example, you suggest that busing is a good policy, it implies that you have reasons that sustain that evaluative conclusion, reasons that express your understanding of that policy and what it is that you see as *of value*. In contrast, subjective preferences, offered in the first person as a claim about what "I" value (want, like, desire), are intrinsically personal, implying no interpersonal standard for assessment and no basis for public discussion.[8] They simply express what appeals "to me." Different evaluations or opinions, taken subjectively, indicate only diversity among preferrers, not disagreement about what can justifiably be sanctioned as good or right.

Of course, we can give reasons for our likes and wants. We can explain our personal preferences, autobiographically. Or we can explain our preferences by identifying the features in them that we find valuable. I can say "I like George Bush more than Mario Cuomo," for example, and explain that preference by referring to Bush's position on abortion and defense spending, his partisanship, and his previous experience in office. I can insist that these are characteristics that I value, and no one need quarrel with me. But if I claim, further, that George Bush is *better* than Mario Cuomo as a choice for president, then I am making a judgment of value that goes further in implying that my reasons for liking Bush call upon interpersonal standards of value that others must acknowledge.

Scholars employing subjective conceptions of value often recognize an equality among opinions that derives from a conception of equality among persons. Social choice theories express this notion by giving equal weight to the subjectively conceived utility of each person they recognize. Opinions (as expressions of utility) can be criticized relative to each person by reference to procedural norms of rationality, but no social or interpersonal standard of value provides a basis for criticism. According to a subjective perspective, good opinions in politics are opinions that can pass a test of procedural rationality, that evidence some desirable bond to the person giving them expression. If value is conceived subjectively, what people need for political expression are the resources to know, in some thick sense, what they care about.

But an ethical perspective recognizes distinctions among opinions by virtue of social standards of justification, expecting more of public opinion. Opinions must be sustainable by reference to justifications that can be offered to others, expressing value judgments that find some social niche. They must provide a basis for challenging alternative contentions about what is good or right. The issue here is not one of knowing what you want—of having formed a preference in that sense—but of having what it takes to make an arguable case. To express themselves or act politically, individuals must not merely "know themselves" but have the resources to express themselves in ways that are compelling to others in society.

When reflecting upon his interviewing experiences, Converse commented that "very many respondents could not understand that a battery of pure opinion items had no objective 'right–wrong' scoring" (1974, 650). This phenomenon loses what oddity it may otherwise have once we conceive of opinion as that which others can and do judge, critique, and even condemn. When we understand opinions as claims requiring justification, claims that compete with alternative views in conveying that which is good or right, we cannot rely on a simple notion of equality among persons to reach a conclusion that one opinion is just as good or valid as any other. Opinions can and will be criticized on grounds that question their roots in facts and values, and citizens will feel discomfort when expressing opinions that they feel may be inadequately bolstered.

The differences between ethical judgments and personal preferences are tied to the kinds of reasons that are offered to sustain them and to how each kind of claim functions in public discourse. Ethical disagreements (disagreements about what is of value or whether some person or act or policy has value) reflect disagreements that can be probed by examining the nature of the justifications upon which they rest: factual premises, fundamental goals, understandings of the consequences that flow from action, and guiding principles of right.[9] Ethical judgments rest on interpersonal justifications and not just on unique personal explanations. They appeal to others and do not just describe the self.

SELF-INTEREST *AS* ETHICS

Justification is always at the root, if not at the forefront, of ethical claims. If we simply begin with that premise, then it is clear that there is no inherent contradiction between self-interested and ethical claims, no reason why self-interest could not be cited in attempting to justify some outcome or action as right. Indeed, if by virtue of serving one's self-interest a political choice could be sanctioned as justified, then there would be no incompatibility between the dictates of self-interest and ethics. Some version of self-interest—that it is "what I want" or "best advances my interest in X"—could be invoked as an ethical argument (or a necessary component thereof), advanced to sustain a judgment that a policy or course of action is right.

I will take a brief look at three different arguments that each try to justify self-interested action as invariably right: Sidgwick's *ethical egoism*, Ayn Rand's *objectivist ethics*, and a *utilitarian* perspective on self-interest. I do not present these perspectives as exemplars of the kinds of ethical arguments we might find people adopting empirically. Indeed, the notion of equating self-interest and ethics in this way may well seem odd if not downright preposterous.[10] The purpose of considering these arguments is, instead, to dislodge any impression that acting on ethical grounds invariably requires setting aside self-interest and at the same time to expose the difficulties encountered by a self-interest-based ethics. Acting ethically does not require a subversion of self-regard or interest, but it does minimally seek a reconciliation of that self-regard or interest with the self-regard and interests of others. It is in the face of this requirement that self-interest-based ethical arguments fail.

Ethical egoism, in a 1907 formulation by Henry Sidgwick, is the view that morally, one ought to pursue one's self-interest. Self-interested choices as self-interested are justified as right.[11] Following Sidgwick's account, this means that an individual should choose from among the alternatives faced the course of action that would yield "the greatest surplus of pleasure over pain" (1981, 121).[12] For the egoist, "a tendency to promote his own interest is the sole possible, and sufficient, justification of all his actions" (Moore 1988, 97) and also serves to justify the moral rightness of self-interest-based action by others. The egoist as actor is doing that which is morally right in choosing actions, in politics or in private life, that maximally advance his self-interest. The egoist as observer or judge of others, must, as such, conclude that they, too, are justified in pursuing their self-interest if he allows that his justification is as valid as one that any person might offer. It does not matter if these self-interests collide. Each is nonetheless justified in pursuing their individual aims. Margolis elaborates the egoist's stance: "The ultimate justification the egoist is prepared to offer is that he is uniquely himself, that what suits *him* justifies his conduct since no one else is, ex hypothesi, sufficiently like him (being different from him) to extend the justification to another in suitable circumstances. . . . [The egoist] does what he pleases, but there may be no other significant regularities that may be singled out [to justify the conduct]" (1971, 180–82). The ethical egoist cites no features of any action or choice when justifying it as good or right. Instead, the egoist defends his choices with the simple justification, "It is what *I* want."

As a public account of what is right, ethical egoism suffers from one primary flaw. Precisely when the egoist would be called upon to justify some action or proposal (when people's self-interests are in conflict) he will, in fact, fail to justify his conduct to others. The egoist, says Kalin, is thus "doomed to frustration . . . because justifying his behavior in terms of [his self-interest] gives an opponent no reason to cease his opposition if maintaining it would be in his own interest" (1970, 82). Falling back upon the subjective plea, "It is what I want," fails to serve as a justification at all. If challenged, the egoist can do little more than continue to repeat that simple claim.

Because of this key problem, it is implausible to expect that contentions about the significance of preference satisfaction will provide compelling justifications for the existence or the form of political action.[13] Self-interest (as given by ethical egoism) fails to provide a sufficient basis for policy choices if we understand those choices as requiring public justification. It doesn't fail *because* the actions are self-interested, however. It fails because subjective claims of want are baldly devoid of any compelling interpersonal basis for support.[14]

But ethical egoism does not exhaust the ways in which someone might try to justify self-interested action as right. Ayn Rand's (1964) objectivist ethics, an ethics of "rational selfishness," sidesteps the problems facing ethical egoism. Her egoist must adopt a "rational, objectively demonstrated and validated code of moral principles which define and determine his self-interest" (p. xiv). That code incorporates three cardinal values: reason, purpose, and self-esteem, which entails that people act rationally, act productively, and aim to "acquire the values of character that make . . . life worth sustaining" (p. 22). According to Rand, these are the "values required for man's survival *qua* man—which means: the values required for *human* survival" (p. 28). According to her ethics, "the *rational* interests of men do not clash" (ibid.); hence no contradiction is implied by a person advocating egoism as the moral stance that each person should adopt. Her version of egoism ostensibly avoids the dilemma of conflicting self-interests and provides explicit (if contentious) standards of moral justification, allowing for the possibility of moral appeal and debate.

Early utilitarian theories, modern welfare economics theories, and social choice theories offer us yet another perspective by which we might sanction self-interested action. This normative vision understands the collective welfare as maximally advanced if each individual strives to attain his or her own interests.[15] An individual accepting this causal theory would view acting on her self-interested preferences as justified because that self-interested preference should merge with others' preferences to produce the collectively best outcome. (Of course, such an individual would argue that others should be acting on their self-interest, too.) Formally, this is not an egoist normative theory because the contention that an action advances one's self-interest is not itself invoked in justification; rather, the justification for self-interested action derives from collective considerations. Nevertheless, action aimed at advancing one's own interests is instrumentally justified as better able to achieve collective well-being.[16]

Armed with Ayn Rand's account of self-interest or this utilitarian justification, self-interested citizens would act not as consumers of the goods of politics but as individual legislators of the good. These ethics do not subvert egoism. On the contrary, each perspective develops a conception of morality that is consistent with its conception of each actor's self-interest and thus gives self-interested action moral justification. By joining self-interested value and moral right they provide self-interest with a language of public justification.

Ethical egoism, Rand's objectivist ethics, and this utilitarian account each confronts—and putatively resolves—a central problem that also guides the development of nonegoist moral theories, namely, the problem of formulating an understanding of self-interest and of morality such that acting on the demands of morality does not require that individuals subvert their own good. Kurt Baier refers to this problem as the "inconsistent triad" where:

1. Proving the rationality of some conduct to someone consists in showing him that it would be for his good.
2. Compelling moral considerations sometimes require us to do things quite irrespective of whether doing them would be for our good.
3. Moral considerations could not require this unless one could be shown that acting from them was rational whereas ignoring them was not. (1986, 13)

The problem of an incompatibility between self-interested and moral dictates is central to ethics, the philosophical solution elusive.[17] Yet (as I will argue shortly) the existence of this dilemma yokes our moral arguments to a concern for individuals and their interests.

We have no good reason to expect that our actual moral beliefs and conceptions of self-interest constitute a system that has overcome this dilemma. It is unlikely that either Ayn Rand's theory or the utilitarian conception capture our ideas about self-interest and morality. Nor is it likely that we always recognize some other form of compatibility between what we care about as individuals and the moral considerations that we find ourselves confronted with. Just as this dilemma permeates moral theorizing, it may well be a basic tension in many individuals' lives. Faced with this tension, the ethical individual does not simply evaluate and act so as to produce a maximally advantageous state of affairs for herself. But she need not ignore her own welfare or autonomy, or integrity when taking moral claims seriously and offering public justifications for her judgments and choices. What public justification requires is, instead, some account of how those interests are to be *reconciled* with the interests of others—some recognition of the requirements of community life.

Thus, while we may find it hard to imagine that human motivation could be at odds with the interests of the self, recognizing Baier's dilemma as a real, empirical dilemma means allowing for moral imperatives that confront and challenge one's interests. Even those who would confine the role of public justification in politics to the context of justifying democratic procedures for the aggregation of citizen preferences (as per social choice theory) must still acknowledge the limits to egoism that this recognition of the collectivity introduces. Consider, in this respect, how we might conceive of the point of justification in the social choice project.

First of all, establishing the goodness of some procedure might be something that could be used in a normative appeal to urge citizen compliance with its outcomes. This, however, makes no sense if people are pure egoists. They would not be willing to accept any particular outcome of a "good" or "just" democratic procedure if it were contrary to their own interests (however defined).[18]

Alternatively, we might see the mass public as never willing to go along with the outcome of any democratic procedure if it contradicts their interests. Questions of justice and good procedure do not matter to these "ordinary" people; they matter to certain disinterested parties in academia or in positions of legislative or judicial power. If these elites can certify procedures as just, then the weapons of the state can be legitimately used to enforce compliance among the egoists (citizens). This appears to be the view of Jules Coleman and John Ferejohn, for example. They suggest that "to assure *voluntary* compliance in a joint venture, cooperation must be to each person's advantage"; that in order to achieve successful collective action, policies "must be coercively enforceable"; and also that "coercion requires justification" (1986, 6). According to this interpretation, there really is no reconciliation problem for individuals but simply different kinds of individuals. Someone with the power to implement collective decision rules cares about justice (a dictator perhaps?), but the rest of the citizens, who will be subject to those rules, do not. Given the egalitarian premises represented in social choice axioms, this elitist interpretation introduces a certain irony—if not an inconsistency—into the social choice project.

If, therefore, advancing a conception of political actors as egoists, social choice theory's own account of the problem of justifying democratic procedures becomes problematic. However, we might, instead, understand social choice theories as saying that citizens are egoists in all circumstances *except* when it comes to questions of democratic procedure. Then they reveal a concern with justice or other standards of moral right and will go along with the outcome of any procedure that in the abstract, does a "good" job of aggregating preferences even if it conflicts with what they want. While this particular division between egoism and ethical concern does admit of a reconciliation problem facing citizens, it is also, of course, ad hoc.

SELF-INTEREST *IN* ETHICS

Acting on the basis of ethical concerns may conflict with acting on the basis of self-interest. What cases of divergence we recognize will, of course, first depend on how we define and study "self-interest" and the demands or dictates of ethics. That given, whether their dictates converge or diverge, and for whom, will also depend on the particular empirical circumstance at hand. In any given context, that which is to someone's material benefit, or maximizes someone's relative advantage over others, or satisfies someone's personal preferences may or may not be what that person or others judge as right or good—may not best advance the welfare of persons taken collectively, may not be best for society under some alternative consequentialist construction, may not be fair, may violate some right, and may not adequately fulfill commitments made to particular people or those made as a citizen.

I will look more closely at the possible divergence and (especially) convergence of self-interested and ethical claims. I will not say anything as yet about the purely circumstantial convergence of self-interested and ethical directives. Nor will I return to egoist arguments that always stipulate a convergence between the dictates of self-interest and ethics. Rather, I will discuss more limited ways in which preserving or advancing one's self-interest might be sanctioned by ethics. For even though some conceptions of good and right might confront and challenge our interests, we may also turn to normative arguments in their defense.

We might begin by defining "self-interest" as whatever it is that an individual values, noting that moral claims of goodness or moral directives of right may conflict with value as *defined* by the self. Wollheim conveys the essence of the subjective claim:

> The wants which the citizens' choices express need not, of course, be selfish or egotistical wants. When a man decides that he wants policy A more than policy B, he may well be moved not just by his own interests but by a concern for the welfare of others. But all the same, in choosing A he is not asserting that others want A, nor that A is in their interests, nor that A would be an ideal solution, nor that A ought to be realized; he would be asserting *tout court* that he wants A. (1964, 76)

This version of self-interest refers simply to that which an individual wants or desires—in rational choice language, that which yields maximal personal utility. It focuses not on personal welfare (as self-regard) but on personal autonomy (as self-definition). Analytically significant here is not the substance of what I value but that it is what *I* value, given recognition simply because it is mine. A disjuncture between self-interested and moral claims will arise if one's personal preference (whether self-regarding or not) fails to coincide with that which is justified as right or good. What I want to do might not be right (according to some persons, some argument, or some government); or what I like might not be that which I or others judge good.[19]

Yet these personal preferences need not be silenced by the din of contradictory moral claims. Our ethics recognizes the moral significance of personal desires *qua* personal, a significance that philosophers have represented by making a distinction between agent-neutral and agent-relative justification (Nagel 1986; Parfit 1984). Agent-*neutral* justifications base judgments of right in reasons that make no special reference to particular persons or their preferences, yielding *impartial* judgments. Bernard Williams describes such justification as requiring

> abstraction from particular circumstances and particular characteristics of the parties, including the agent, except in so far as these can be treated as universal features of any morally similar situation. . . . The motivations of a moral agent, correspondingly, involve a rational application of impartial principle and are thus different in kind

> from the sorts of motivations that he might have for treating some particular persons (for instance, though not exclusively, himself) differently because he happened to have some particular interest towards them. (1981, 2).[20]

Agent-*relative* justifications, in contrast, pay particular attention to the identity of particular persons, endorsing *partial* judgments. Focusing attention on the nature of social roles and relationships, some relative justifications sanction preferential treatment or concern for people who stand in particular relationships with the agent, under any number of descriptions, including friends, spouses, children, colleagues, and in some contexts, fellow citizens (call these group-relative). Other relative justifications give special attention to the dignity and worth of individuals and, consequently, to the subjective preferences of the agent (call these self-relative). It is by making such arguments about the dignity and worth of individuals that self-interest-as-subjective-preference finds its own normative voice.

In an important sense, however, both self- and group-relative justifications pay attention to the (judging, evaluating) self. Group-relative justifications do so by locating the self in social roles and relationships, giving special recognition in moral argument to a person's particular concern for his family, friends, colleagues, and so on. When set against impartial dictates, a concern for these individuals (or one of these individuals) is something of particular importance to the self. For example, what I or others judge fair might conflict with what I owe to my family; or my concern for some person *qua* person might conflict with my concern for another person who is my friend. Group-relative arguments about duty and obligation speak in defense of these partial concerns.

When set against subjective wants, however, those same partial concerns can be restrictive to the self. What I want may conflict with what I owe to my family or friends, or my interests may conflict with those of some group to which I have ties.[21] And these same subjective wants may also conflict with moral right defined in impartial terms. I may want to keep the wallet I found in the corridor, to forgo paying taxes, or to hire persons who look just like me; but these may not be the actions that an impartial morality sanctions as right.

Confronted with such conflict, the agent might defend acting on the basis of her personal preference by drawing on self-relative normative arguments—commonly, by appeals to a principle of autonomy or freedom.[22] Consider an example. A homeowners group proposal, if accepted, will dictate that each homeowner must build the same kind of fence. As a single homeowner, I may have a purely personal preference for a fence different from that described under the proposal. In fighting the proposal, I would not back away from an understanding of my preference as subjective by arguing that my fence preference is the preference that everyone should adopt or that it is not just my preference but is better in some way (of course I might, but the point here is to assume I won't). But in recognition of the conflict between my preference and the proposal, I might appeal to some understanding of property rights or rely on some other general (and agent-relative) argument about autonomy in an effort to protect my fence preference and defeat the proposal.

Partiality toward oneself has received a great deal of recent attention among philosophers (e.g., Nagel 1991; Railton 1988; Raz 1986; Scheffler 1982; Williams 1973, 1981, chaps. 1, 3). For example, Scheffler (1982) has argued that morality must recognize an "agent-centered prerogative" that releases people from acting in accord with the dictates of an impartial moral rule when it conflicts with important personal projects and commitments. He describes the potential conflict:

> People typically acquire and care about their commitments quite independently of, and out of proportion to the value that their having them and caring about them is assigned in an impersonal ranking of overall states of affairs. . . . Each person has a point of view, a perspective from which projects are undertaken, plans are developed, events are observed, and life is lived. Each point of view constitutes, among other things, a locus relative to which harms and benefits can be assessed and *are* typically assessed by the person who has the point of view. This assessment is both different from and incompatible with the assessment of overall state of affairs from an impersonal standpoint. (pp. 9, 56)

Incorporating such self-partiality within a moral argument allows a conception of subjective self-interest as "important personal projects and commitments" to bear some moral weight when in conflict with other moral claims. Thus, my commitment to nonviolence—rather than nonviolence per se—is accorded moral status. Or my hard-earned money is given status as mine, something central to my plans of achievement, not to be transferred unconditionally to meet a neutrally justified goal.

Some components of our ethics seek to bind the community of individuals, construing moral right in terms that are authoritative and transindividual, placing demands on people that may constrain them from pursuing their personal interests, whatever they may happen to be.[23] Other components seek to recognize the dignity and worth of each individual and of their particular intracommunity ties, providing individuals with a voice of dissent against the dictates of an impartial morality. By distinguishing among impartial, group-partial, and self-partial claims, we can capture the tensions that may erupt. What some person, because of her individuality, particular goals, and life experiences, has come to value may diverge from other partial claims (what one must recognize as owed to others by virtue of particular ties and relationships with them) or from impartial claims (what "anyone" must recognize as right or good). A member of Congress, for example, might judge a policy as best for *his or her* constituency (a group-partial claim) but not best for *any* constituency or for society writ large (impartial claims), nor for his or her own interests. In the face of such conflicts, personal interests are not inherently trumped by any properly "moral" claim.[24] If impartial or group-partial moral dictates contradict personal preferences, then some resolution must be sought; and such resolutions may delineate trade-offs that are defended as *justified*.[25]

Thus, by appealing to principles of freedom or autonomy, we may defend acting on the basis of our purely personal preferences when doing so conflicts with that deemed right on other grounds. My preference may not be that which I or others deem good or right, but it is nonetheless morally significant *as mine*. We embrace our own individuality in calling upon such principles, while acknowledging the individuality of every other citizen, as well.

But we might instead deny our uniqueness or individuality when speaking in defense of our interests. People may (and perhaps often will) appreciate what they value as *valuable*—commendable for reasons that others should acknowledge. Personal "preferences" may not be purely personal. Our member of Congress, for example, may *want* what he judges best for his constituency or deems best for society as a whole. Under such circumstances, value as assessed by the self may be value defended as "good"; actions of interest to the self may be actions defended as "right". Any distinction we might want to make between an individual's conception of her interests and her conception of moral good or right could collapse.[26] Consider some examples.

Parents who value what they deem good for their children may argue, further, that what is good for their children is good for any child or good for all of the children who live in their community. For example, in expressing a wish that their child be educated in small classes, parents may not be merely offering a personal preference. They may contend that others must recognize—and government must protect—such education as a *good*. Or, while someone may favor a policy that prohibits logging in United States parkland, they may further defend that position as *right*.[27]

Equally well, one's self-regarding interests may themselves be defended as good or right. For example, people might advocate public health care provision while defending that policy through arguments citing "urgent interests" (Scanlon 1975), needs, or rights. Or, consider the poor and the wealthy each turning to government in search of economic advancement. They may not *simply* be seeking to advance their "self-interest" (*qua* material gain). Some argument about distributive justice might be invoked to justify the efforts of the former (though not the efforts of the latter). Some utilitarian account, calling upon supply-side economics, might be invoked to justify the efforts of the latter (though not the efforts of the former). Even if we bring to our political inquiries some self-regarding definition of self-interest, we are not defining something that falls outside of ethics.

Thus, while some understandings of what is good or right will sanction policies that do not maximize a person's self-regarding concerns or that fail to coincide with a person's personal likes or wants, people may also turn to normative arguments in defense of their interests. They may defend acting on their purely personal preferences or their concern for particular others. They may defend what they value as "good" or "right". Absent any smooth resolution of Baier's "inconsistent triad," we will find our interests to be both challenged and championed by our ethics.

CONFRONTING THE ETHICAL CYNIC

"All this talk about ethics is baloney," says the ethical cynic. The cynic recognizes the existence of moral views—expressions of concern about other's interests, assessments of goodness and right, claims of virtue and obligation. But, to use Kenneth Thompson's characterization of the "realist," the cynic sees these as "calculation, rationalization and self-justification concealing the taint of self-interest" (1987, 12). Moral concepts only muddy the picture of what truly can only be understood as the operation of self-interest. Individuals who purport to take moral values seriously are either insincere, or sincerely misguided, or simply confused. But that does not mean that researchers should be taken in.

I have several responses to the cynic. First, if moral claims are all rhetoric and mystification, of no significance to anyone, then how could they become a strategic tool? Why is self-interest something that must be concealed? How could normative arguments facilitate self-interested achievements? If everyone were purely self-interested and immune to moral claims of value, they would be of no strategic value whatsoever. Why would we "speak the language of morality, impressing upon our fellows their duties and obligations, urging them with appeals to what is right and good, if we could speak to the same effect in the language of prudence, appealing to considerations of interest and advantage?" (Gauthier 1967, 460). Ultimately, then, if Grofman and Feld are right that "politicians obviously recognize that appeals to 'right', 'good', and 'fair' policies have some political appeal" (1988, 572), then at least *some* of the people whose evaluations and choices we study cannot be ethical cynics themselves.

Second (as I have already argued), there is no simple division between moral and self-interested claims. Self-interest is not something that always lurks sneakily beneath moral rhetoric. In many cases, particular interests are a critical and acknowledged component of the moral justification itself. In such cases, in fact, one problem is to provide a sufficiently compelling argument about the interest involved, so that the preservation or enhancement of the interest can be given a rights-based justification or one that makes use of liberal ideas about need or harm.[28] In other circumstances, of course, the justification for right action will not make reference to the interests of the actor, and the conjunction of interest and moral value can be a problematic one. One variant of this problem cites the importance of intention to the judgment of acts and persons. The evaluation of acts and actors can be sensitive to the imputed intentions of the actor, recognizing, for example, distinctions between an act of charity designed to enhance one's political popularity and one undertaken with charitable intentions. If the overlap between moral and self-interested value is a transparent one, then even the "sincere" charitable actor might want to conceal the coincidence of personal gain.

None of this means that all moral claims are necessarily sincere. *Any* policy given sanction as "right" will always have consequences that affect people differently. If we take self-interest to be concerned with self-regarding consequences, then we can always divide people into categories according to some objective understanding of how favorable or unfavorable the consequences are for them. It is this inevitable overlap that raises the specter of ethical claims advanced purely in the service of one's self-interest. But as I see it, this establishes nothing except, perhaps, to point to a problem that moral justification is invoked to solve: It is because we are different and yet interdependent that we call upon moral argument to justify political outcomes, seeking solutions upon which all can agree. What matters to the sincerity question is not the convergence or divergence between self-interested and ethical dictates but the consistency with which ethical principles find application across circumstances under which they might be invoked:

> They must not recognize some particular principle as important and weighty in one case and then wholly ignore it or treat it as inconsequential in another, refusing to apply it even though no other, competing principle of comparable power is involved. That kind of incoherence is hypocrisy; someone who is unwilling to apply a principle consistently does not really accept it, and so has no genuine justification even for those decisions in which he claims to be following it. He cites the principle then only to disguise the fact that his decision has some other, less creditable, basis.[29]

Moral arguments might be used by some simply as a tool for the advancement of their personal interests; but such insincerity does not simply follow from the recognition that any claim of ethics can be described as advancing some, and not others', interests. We should recognize the necessary overlap between these consequences for "self-interest" and moral sanction and subject it to careful theorizing and investigation.[30]

Scholars typically solve this problem of overlap by defining it away—commonly by stipulating that explanatory priority belongs to explanations based on self-regard (the *insincerity strategy*). For example, Citrin and Green classify as self-interested "those choices that enhance the actor's own welfare regardless of whether others, including his reference group, benefit or suffer" (1990, 6). And Schlozman and Tierney reason as follows: "When professors plead for a strong university system in the name of . . . general benefits, there is an ambiguity to their position. On the one hand, they surely believe sincerely in the importance of higher education to the good of the nation; if they did not, they probably would not have become professors. On the other hand, their own livelihoods are at stake. Hence, it is not unreasonable to consider their seemingly public-spirited rhetoric as merely a cloak for the cynical advancement of their own private interest" (1986, 34). If a moral justification is available for actions or policies that advance the interests of the self, the former is deemed irrelevant by definition. Others place explanatory priority on competitors to self-interested explanations (the *sincerity strategy*). For example, in Sears, Hensler, and Speer's "symbolic politics" alternative, "whether or not the issue has some tangible consequences for the adult voter's personal life is irrelevant" (1979, 371). In such cases, so long as evidence for an alternative to self-regarding explanations is available, the existence of any self-regarding benefit is immaterial.[31]

Aside from the fact that they simply define away the problem, each of these stipulative strategies is vulnerable on other grounds, as well. The insincerity strategy has potent normative implications since *selfish* is itself a normatively loaded term. Poor people supporting poverty programs, women supporting abortion rights, disenfranchised groups seeking the vote, and slaves seeking freedom cannot be "sincere" in this view. Only the wealthy, the male, the franchised, the slaveowners can express a "disinterested" point of view. The sincerity strategy, in contrast, mutes a number of questions about how—and for whom—self-regarding and moral claims overlap. Especially if some moral claims are insincere, perhaps, but even if all moral claims were sincere, we would want to know how features of society (historical traditions, contemporary institutions, political elites) constrain what constitute "good reasons" or "acceptable justifications" so as to advance the interests of some and not others.[32]

CONCLUSION

Viewing citizens as ethical actors is not the perspective of a hopeless idealist. Though rejecting a conception of politics based wholly on self-interest, it does not deny the significance of personal preferences and self-regarding interests or imply a blanket acceptance of all normative justifications as sincere. Nor does it wander off into other absurdities by, say, denying the reality of political conflict and controversy and presuming a citizenry oriented happily toward the pursuit of common interests. It means regarding each citizen as an individual with her own unique hopes and desires who is at the same time joined with others, part of and continually giving shape to a common social and political life. It sees in political diversity and controversy not just conflicting interests, but also competing claims about what "we"—unique individuals, linked to particular others through social roles and relationships, and together forming a single citizenry—*ought* to do or seek. It challenges us to bring to political inquiry an account of interests and ethics that allows us to explore how they merge and diverge, an account that neither denies the real concerns of individuals nor disregards the exigencies of society and the requirements of communal life.

When judged from this perspective, simply expanding our notions of interests or utility to include "global values and group identification"; "the desire to maintain an established pattern of social relations or to defend distinctive cultural values and practices"; or "a concern for the welfare of reference groups or the pursuit of solidarity, status, self-respect or cultural hegemony"[33] is not a sufficient response to the failures of the economic approach. We cannot just expand our understanding of what an individual might value without recognizing how these values might uniquely function in sustaining political claims. If we reject the subjective account of value as an adequate representation of our experience of valuing, then we must recognize that evaluative judgments vary in intelligibility to others and in the force of the justifications that sustain them.

At the same time, going beyond self-interest in accounting for public opinion and political choices requires more than the application of typologies which juxtapose self-interested and non-self-interested concerns. Characterizing politics as ethical does not invoke "something that is not self-interest"; and the project of studying ethics in politics does mean adopting an ethical account of the bases of political judgment and choice to serve as a competitor to a self-interested account. Such typologies impose a dichotomy that is false, erasing from our inquiries the very questions about interests that are raised by an account of politics that invokes ethics, namely, to what extent *do* citizens find their interests to be challenged by what they or others deem good or right? And how are such contests resolved?

In practice, invoking ethics in politics means expanding our understanding of the criteria by which the citizen may judge political options, recognizing that together with any consequences for one's "self-interest" are features that one's ethics might commend or condemn. We might, for example, bring survey or intensive interviewing techniques to bear on these questions by asking people to judge persons or policies both on relevant moral grounds and in terms of the consequences for their lives.[34] This would allow us to investigate whether similarly situated individuals vary in their perception of whether some policy advances their interests (as they themselves define them); whether people differ in the extent to which they see any policy or option as meeting some criterion for justification (e.g., some, but not others, might see a policy as violating a particular right or causing a particular harm); and whether there are variations among people in the weight they give to different moral criteria when forming their policy opinions (e.g., appeals to autonomy or obligation might be more significant among those whose autonomy or commitments to others are in fact challenged by some policy or proposal, compared to their significance among those situated otherwise).

If politics articulates interpersonal standards of value and justification and if political expression requires justifying one's views as good or right, then we must recognize these judgments as significant to political behavior research. This challenges empirical researchers to recognize that doing social science *does not mean that we should avoid anything remotely* "normative" but, in a way, the reverse. Public standards of value found in the discussions of moral and political theorists must be incorporated into empirical work, finding service in an explanatory role. As we proceed with our political inquiries, we will want to develop an account of self-interest that is faithful to the concerns of real people and also a sophisticated account of the conceptions of good and right to which our politics gives voice. These developments are necessary if we want to expand our understanding of how interests and ethics function in a politics that requires public justification.

Notes

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1. Relatedly, some speak of "a taste for honesty" (Frank 1990, 93) or argue "[that] individuals have different tastes, that one man's meat is another's endangered species" (Schlozman and Tierney 1986, 23).

2. Interests are usually not equated with desires or wants but, rather, functionally related to them. A policy or action is in some person's or some group's interest if it is instrumental to the attainment of something that they (individually or collectively) desire or want (Barry 1965; Held 1970).

3. Those arguing recently for a conception of politics that deemphasizes preference aggregation and emphasizes debate over, and judgments about, public goals and policies include Cohen 1986; Dagger 1986; Grofman and Feld 1988; Mansbridge 1980; Sagoff 1986; Sunstein 1990; and Waldron 1988.

4. The normative questions of politics are not, however, merely the questions of ethics asked in politics. Moral justifications define social dictates but not necessarily political solutions; and "political", not "ethical", justifications speak directly to the functioning of political institutions and to the bases and scope of government action. While I will speak of "ethics," much of what I have to say refers more generally to publicly justified (or justifiable) judgments, whether political or moral.

5. Of course, ethical judgments may not always be accompanied by public justifications; but they are implicit and may be called for in a context of discussion and debate. Clearly, public justification will be most evident in some institutional settings (e.g., Congress, courts, juries, public meetings).

6. The structure of utilitarianism is identical to that offered by rational choice theories' calculi of decision tying maximum personal utility to choice. Whereas the utilitarian looks to social utility when formulating the judgment of right, the rational actor looks to personal utility in determining what is the best alternative. Although their theories of value divide them, their formal framework is otherwise the same.

7. While I will speak of good versus bad and right versus wrong, these are not the only evaluative categories important

to ethics and thus to the general conception of evaluation I am advancing here. Our evaluative language is much richer than is revealed by these dichotomies, as those working in a virtue-theoretic tradition have emphasized (e.g., Meilaender 1984; Pincoffs 1986).

8. On this point, see also Gaus 1986 and Parks 1982. By saying that ethical claims of value are different from personal desires, I am edging toward issues of major philosophical controversy. That controversy centers on the ontological status of value and the epistemological status of judgments of value. The philosophical literature on these issues is vast, fluid, and replete with many terminological nuances. One recent review addresses eleven relevant "isms" (Sayre-McCord 1988, intro.). Here, I am simply trying to sketch distinctions we might find empirically important, rather than adopting one or another of the positions in these philosophical controversies. I give closer attention to these issues in Stoker 1990.

9. This is so whether evaluative conclusions are conceived as resulting from deliberation about justification or not; for even if they arise from other processes (e.g., learning processes), justificatory premises and beliefs may hold them intact.

10. Numerous scholars have contended that in democratic politics, self-interested claims simply cannot be advanced to justify political outcomes. Anderson, for example, suggests, "One cannot justify a policy recommendation on the ground that 'it would make me and my friends richer'. However refreshing the candor of such an argument may be, it does not and cannot stand as a legitimate warrant for public action" (1979, 718). Elster maintains, "There are certain arguments that simply cannot be stated publicly in the political setting. In political discussion, it is pragmatically impossible to assert that a given solution be chosen simply because it favors oneself or the group to which one belongs" (1983, 35). Citrin and Green contrast the "normative and institutional contexts" of political and economic choices, suggesting that in politics, "people are frequently expected to decide issues on the basis of principle, to make decisions about what is right or wrong, . . . to behave in the general interest and to justify their choices in ethical terms" and that "selfish actions are often viewed as illegitimate" (1990, 20).

11. Ethical egoism does not assert that people *are* motivated by the singular pursuit of their self-interest (however understood). Nor does it advocate that people adopt, merely for themselves, a self-interest maximizing principle of action. Such a person may be acting on principle, but not a moral one. Ethical egoism requires that a person adopt the self-interested maxim as alone prescribing morally right action and, further, advocate that maxim for all persons. The ethical egoist's justification of self-interested action as "right" requires acknowledging the force of those reasons for defining the right action of others. This extension to others of the egoist's justification reflects what moral philosophers refer to as the universalization of moral judgment. Without such universalization, the egoist arbitrarily restricts the set of persons given access to his justification, saying, "I am justified in doing what I want, but you are not justified in doing what you want." With this arbitrary restriction, the view is not a moral one; and if some restriction is justified, the theory is no longer ethical egoism.

12. For Sidgwick, *pleasure* is "taken in its widest sense, as including every species of 'delight', 'enjoyment', or 'satisfaction'," where "all qualitative comparison of pleasures must really resolve itself into quantitative" (1981, 93–94). Ethical egoism could, of course, be formulated nonhedonistically. (Sidgwick was not writing as an unequivocal advocate of ethical egoism; he also found utilitarianism rationally compelling.)

13. When considered in empirical terms, ethical egoism's implausibility is also found in its inability to account for significant components of our ordinary moral thinking and debate, namely, the components that identify value in actions or states of affairs not just on the grounds that it "advances my interests" or "is what I want" but on the grounds that it is "fair," advances collective welfare, and so on.

14. In contrast, we can, for example, envision the following (Pareto optimizing) claim being offered in defense of some policy: "It will be to my benefit and not impose any costs on anyone else." Notice that ethical egoism does conceive of citizens as agreeing that "each should pursue his or her interests." That general contention can ostensibly be interpersonally justified. But when the egoists find themselves with incompatible wants, they are left without any argument about how to proceed.

15. The prisoner's dilemma, a strategic circumstance that contradicts this normative conclusion, is a "dilemma" defined within this framework. Rational self-interested actors will not opt for actions that are collectively best. Because the prisoner's dilemma outcome is also not individually optimal, this context often raises a different claim about the convergence between morality and self-interest (one also found in utilitarian thought), namely, that self-interested people may best achieve well-being by not always directly aiming to achieve their own well-being. Axelrod's (1984) work on iterated prisoner's dilemmas might be seen as suggesting this conception of self-interest and morality. Strategies that are "nice" and "forgiving" are evolutionarily successful.

16. Sen (1987) focuses some (critical) attention on the empirical accuracy of this kind of claim. Hirschman (1977) traces the historical development of the idea.

17. This tension is vividly evident in Brock's definition of *morality*: "Morality in the narrow sense: includes all those principles that restrict the individual's personal goals and his advancement of his self-interest. Morality in the broad sense: is 'the art of life', that is, the precepts instructing people as how to live, and what makes for a successful, meaningful, worthwhile life" (1988, 570). In other words, morality is both limiting individuals' pursuit of their "self-interest" and telling them what it is!

18. Social choice theorists might, of course, claim that the outcomes of a good or just procedure will always be in the interests of each citizen. But this would be simply denying the possibility that people will ever face a problem of reconciliation.

19. The latter, among other things, captures purely expressive conflict. It recognizes that we sometimes want to preserve our opinions from outside scrutiny and remove them from a context of public justification. We sometimes say, "It's just my opinion." We fall back on a subjective account of value that while it may or may not be rooted in some principled objection to an objective metaphysic, has the main consequence that it takes our opinions out of the public spotlight, attempting to protect them from the criticism of others.

20. Williams is writing here about "the moral point of view" and does not use the term *agent-neutral justification*. When writing about these issues, others speak of judging from the "impersonal standpoint," of "impartial justification," and so on.

21. Altruism (persons' forgoing their self-regarding interests in favor of the interests of others) is captured by this tension between self- and group-partial claims. As such, contrasting altruism with egoism does represent the potential for conflict between some claims of morality and of self-interest. But they are conflicts of a particular kind and presented by a particular, and perhaps narrow, collection of circumstances, namely, those where we can establish a discrepancy between self- and other-regard, where gain to the self may be denied to enable others to profit instead. Altruism is also particularly suited to addressing the philosophical question whether it is ever rational to deny one's self-interest (e.g., Nagel 1970). Altruism finds its niche here, not in somehow representing the "essence" of morality.

22. Speaking more generally, partial claims justified in agent-relative terms can be formalized into rules or principles, which are then justified in agent-neutral terms. Acting on the basis of one's subjective preferences can, as such, be neutrally justified if universalized for any person or persons so described. "It's what I chose" could (in some specific context) be defended as important, universalized for any "I." Similarly, while group-relative justifications take note of the particular

identities of persons by virtue of socially defined relationships, they also can be universalized to refer to any particular set of persons so described (Nagel 1986).

23. Such claims may be advanced in the name of maximizing social welfare (as in utilitarianism) or some larger set of intrinsic goods or in the name of some conception of community, nature, God's will, or the like.

24. A philosophical caveat: How to give weight to partial and impartial claims of value is always explicitly or implicitly resolved by philosophical theories of ethics, but they do not always recognize partial claims as "moral." There are two bounding perspectives. The first sees morality as yielding dictates grounded in impartial justification (as with a utilitarian morality), with partial concern for the self or others lying outside morality. If the interest of the self does not coincide with that deemed morally right, a contest between self-interest and morality ensues. The second seeks to bring partial claims within the bounds of morality, giving self-referential partiality the force of moral justification when it confronts other moral dictates. While the first sees the individual as asking, "Why should I be moral?," the second sees the person as asking, "What does morality require?" According to either perspective, the person may reject group-partial or impartial dictates in deference to his self-interest, but the second might sanction such choices as moral. Still other ethical theories might accept some forms of partiality as moral (e.g., partiality to one's children) but exclude partiality to oneself. In such cases, the individual is still asking, "Why should I be moral?" However, a distinction is maintained between self-relative and group-relative partiality. The distinctions among self-partial, group-partial, and impartial judgment are important whether the lines of confrontation are drawn across, or within, the boundaries of "morality."

25. If the individual's personal latitude is too narrowly constrained, individual integrity (Williams 1973) and autonomy (Raz 1986) may be threatened. Yet widening that preserve draws accusations of self-serving egoism: "In defending the legitimacy of agent-relative principles we must guard against self-deception and the escalation of personal claims simply to resist burdensome moral demands. It is not always easy to tell, for example, whether a morality that leaves extensive free space in each individual's life for the pursuit of personal interests is not just a disguise for the simplest form of badness: selfishness in the face of the legitimate claims of others" (Nagel 1988, 170). Scheffler imbeds the agent's prerogative to pursue his own commitments in an otherwise demanding ethics, combining strong consequentialist claims on an individual's attention with a special preserve where the individual's own conceptions of the good may predominate. This hybrid between a strict consequentialist and a purely rule-based approach flips the usual emphasis of the latter. Rule-based deontological views (like most accounts of liberalism) often narrowly demarcate the zone of moral constraint, leaving a wide field of individual prerogative.

26. The distinctions among partial and impartial judgments depend on the identity of the evaluator, not on the substantive reasons underlying a personal or a moral evaluation. As such, there is no inherent conflict among the policies or outcomes that each evaluative perspective may sanction as best: the evaluation of the self *qua* self, may coincide with the evaluation of the self *qua* located in some role or relationship and of the self *qua* person. In other words, it is entirely possible to conceive of a person's own evaluative claim as capable of being supported by group-relative or -neutral justifications.

27. Lurking in these examples are further possible distinctions. For example, we might find important a distinction between people who value something *because* it is good or right and those who value something for some other reason ("It happens to be what I like") but argue its moral or political significance on the basis of its goodness or rightness.

28. These are, of course, only compelling as liberal standards and in a liberal society such as ours.

29. Ronald Dworkin to the Editor, *New York Review of Books*, 1991. p. 65.

30. The question of sincerity will not be easy to settle empirically. First of all, we would need data that meet the comparative requirement set out in the text. Further, the consistent application of a principle or rule does not imply that we will observe a series of judgments or decisions each manifestly instantiating that rule. Countervailing moral considerations particular to certain circumstances may undermine any simple principle-decision correspondence.

31. Things are sometimes murkier than this—at least in part because of the statistical methodology often used to study these questions and the corresponding application of statistical controls. Nevertheless, the conjunction of self-interested and non-self-interested reasons is not itself usually studied (but see Kinder and Kiewiet 1981; Tyler 1990).

32. Examples of systematic convergence between interests and ethics abound. Individuals may enter adulthood with or without the character that our society rewards, an aspect of what Nagel (1979, chap. 3) and Williams (1981, chap. 2) have called "moral luck." The American moral culture defines the maternal role as more constraining to women than the paternal role to men, delineating gender-specific trade-offs between the interests of selves and duties toward children.

33. Quoted values come from Citrin and Green 1990, 11, 17, 23.

34. We might also pursue these questions using observational methods, albeit in a more limited fashion, by applying to a set of public policies some objective coding scheme defining whose self-interests are advanced and what moral arguments might be mustered to sustain support or opposition. Making comparisons across policies would allow us to observe how support varies with the interests that are served by them and with the moral justifications that sustain them. For example, a policy that is fair according to one criterion of fairness may receive more support than policy that is fair according to another criterion of fairness. Some might never act against their self-interest in supporting one or another of these policies, while others may do so in support of one and not the other; and so on.

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HEGEL'S THEORY OF MODERN BUREAUCRACY

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I make three contentions. First, Hegel's theory of bureaucracy is as comprehensive as Weber's ideal type of bureaucratic organization. I argue this by making a sociological reformulation of Hegel's model and comparing it with the Weberian paradigm. Second, Hegel adapts the Aristotelian category of practical judgment in characterizing the bureaucratic activity as subsumption. *This characterization is contrary to the dominant view that bureaucracy embodies mere instrumental rationality and solves some difficulties in Hegel's political thought. Third, Hegel's conceptualization can contribute to a liberal theory of bureaucracy that apprehends modern political reality more adequately than the skepticism of classical liberals and contemporary libertarians about bureaucratic organization.*

Bureaucracy, a conspicuous political institution in the modern state, is also a major theme in contemporary sociological literature. Hegel formulated the first theory of modern bureaucracy in the *Philosophy of Right* (pars. 287–97 [1976]).[1] However, this work has not exerted much influence on the sociology of bureaucracy due to the philosophical way of treatment. Instead, Max Weber's ideal type of bureaucracy (Weber 1978, 217–26, 956–1003) has had a pervasive influence in the development of the sociological tradition. Weber's conceptual and historical analysis provides a comprehensive framework for the empirical study of bureaucratic organization, though his analysis has been modified in details (Luhmann 1982, 30–41). In its normative dimension, the Weberian paradigm assumes that bureaucratization expresses the technical rationalization of modern politics. This normative assumption underlies critical theorists' critique of instrumental rationality thesis (Habermas 1970, 1984–89, vol. 2).

I shall demonstrate that on the level of institutional analysis, Hegel's characterization of the structural attributes of bureaucratic organization is as comprehensive as Weber's ideal type. The affinity between Weber's and Hegel's theories of bureaucracy has been observed by Avineri (1972, 160) and Jackson (1986),[2] but a more theoretical treatment is needed beyond the mere comparison. I shall also argue that on the level of normative evaluation, Hegel proposes an interpretation of bureaucratic activity entirely different from Weber's, one that may be articulated into an alternative to Weber's instrumental rationality thesis. Hegel shows that the rationality of modern bureaucratic activity is not technical but practical (in the Aristotelian sense); and he correlates bureaucratic and judicial activities. The implications of this view will be analyzed by way of Gadamer's theory of hermeneutics.

The recent trend to depoliticize Hegel's political philosophy also urges a reexamination of his theory of the state. Even the influential studies by Ritter (1982) and Riedel (1984) have focused too much on civil society and not enough on Hegel's theory of the state. Since Hegel sees bureaucracy as the main governing organization in the modern state, his contribution to the sociology of bureaucracy is impeded by this depoliticization tendency.[3]

A more philosophic reading of Hegel's *Philosophy of Right* has been launched by Michael Oakeshott. In his seminal *On Human Conduct* (1975), Oakeshott distinguishes two concepts of the modern state: the paradigm of *societas* (the state based on the rule of law) and the antithetical *universitas* (enterprise or purposive organization). Unlike most commentators of Hegel's political theory, Oakeshott identifies Hegel's theory with the concept of *societas* (pp. 259–63), paving the way for a liberal reinterpretation of Hegel's thought that is also philosophically interesting (Smith 1989a, 1989b). However, Hegel's view that bureaucracy is the principal governing organization poses a major difficulty for Oakeshott. Like other classical liberals, including Tocqueville and John Stuart Mill, he is deeply skeptical about bureaucracy. For Oakeshott, bureaucracy is one of the prototypes of *universitas* (1975, 267). As a result, he emphasizes Hegel's theory of recognition but overlooks Hegel's theory of bureaucracy.

I shall show that Hegel's embrace of bureaucracy can be explained within the liberal framework. I shall also argue that for Hegel, bureaucracy is not only compatible with, but indispensable to, the modern constitutional state in view of the disruptive effects of civil society. In this respect, his ideas may contribute to a liberal theory of bureaucracy.

THE IDEAL TYPE OF BUREAUCRATIC ORGANIZATION

Hegel discusses bureaucracy in his treatment of the executive power (*Right*, pars. 287–97). The discussion is brief; but this does not imply that Hegel considers bureaucracy less important than other constitutional powers. In his treatment of the executive power, Hegel analyzes modern bureaucracy as a form of political organization, its constituent members, and its relation with the social environment.

Hegel describes the executive power as follows:

> There is a distinction between the monarch's decisions and their execution and application or, in general, between his decisions and the continued execution or maintenance of past decisions, existing laws, regulations, organizations for the common ends, and so forth. This task of *subsumption* conceptualizes the *executive power* in itself, which includes the powers of the *judiciary* and the *police*. It has immediate bearing on the particularity of civil society and makes the universal interest valid over particular aims. (*Right*, par. 287; emphasis original; trans. based on Knox's)

For him, the executive mediates between the state and civil society. Its distinctive function is to execute and maintain existing legal norms. In its relation with civil society, the executive upholds universal interest among particular pursuits. Its specific mode of activity is designated as *subsumption.*

The organizational characteristics that Hegel attributes to the executive indicate that it is a bureaucracy in the modern sense:

1. It has a functional division of jurisdictions in accordance with different tasks. (*Right*, par. 290)
2. The divided branches are articulated by the principle of hierarchy. (par. 290)
3. The office is separated from its incumbent; there is no natural or necessary link between them. (par. 291)
4. Since the operations of bureaucracy have been objectified, bureaucrats need not be geniuses. The criteria for entering the civil service are knowledge and proof of ability through examinations, not birth or status. (par. 291)
5. Modern bureaucracy tends to diminish the traditional type of social stratification, because talent becomes the main criterion of recruitment. It is the most appropriate administrative organization for an egalitarian society. (par. 291)
6. Bureaucrats live off the fixed salary remunerated by the state in order to be independent from external influences. They should exercise their authority according to the common good. (par. 294)
7. The main advantages of a centralized bureaucratic organization are maximum simplification, speed, and efficiency in handling state affairs. (par. 290, add.)

Concerning civil officials, Hegel designates them as the "universal class" because the end of their activities is to realize the universal interests (*Right*, par. 205). Their universality is contrasted with the pursuits of the particular interests by the members of civil society. Civil officials are the pillars of the state because they are endowed with the highest political consciousness and knowledge about public affairs (par. 297, add.).

As to its scope of jurisdiction, bureaucracy has two functions. On the one hand, it superintends the activities of corporations in civil society so that the latter do not degenerate into egoistic interest groups. On the other hand, the highest officials constitute the advisory committee for the monarch and report to him the concrete situations of public affairs for his final decisions (Hegel, *Right*, pars. 283, 289).

Hegel is also aware of the hazards of modern bureaucracy and sees it resulting in two major problems. First, if the functional division of labor is excessive, incumbents' activities become mechanized. Bureaucrats therefore need an ethical education to cultivate their capacity to evaluate public affairs from universal perspective (*Right*, par. 296).[4] Second, bureaucrats must be accountable for their actions. Hegel proposes two methods of control, one internal and one external. Internal control consists in the ethical education and organizational discipline of bureaucrats, external control in supervision by the sovereign and the corporations of civil society (pars. 295, 297).

These are the sociological aspects of Hegel's theory of modern bureaucracy. Table 1 compares his model with Weber's ideal type. It is clear from the table that Hegel and Weber agree about most organizational characteristics of modern bureaucracy, the exceptions being criterion for recruitment, type of stratification, and mode of activity. The disagreements will be dealt with further on; here we are interested in the issues on which they agree.

Hegel and Weber recognize the novelty of modern bureaucratic organization. For both of them, the emergence of the modern state from feudalism is a gradual but irreversible process by which sociopolitical relations have become both institutionalized and impersonalized. Under feudalism, aristocrats appropriated administrative positions and resources as their own privileges. During the era of absolute monarchy, feudalism faded away and was replaced by the modern administrative apparatus (Weber 1978, 983–87). This course of political change, which culminated in the French Revolution and its aftermath, was the concern of many nineteenth-century thinkers (Bendix and Roth 1971, 131–40). The controversial question was whether the newly emerging bureaucratic state was different from the traditional bureaucratic empire. Tocqueville, in a well-known criticism, regards the modern centralized state as the result of the "leveling-up," or equalization process (1955, 81; 1966, 10, 690). He emphasizes the continuity between the ancient regime and the modern state, thus undermining the new source of legitimacy declared in the French Revolution. From the Tocquevillian perspective, the modern state with its centralized bureaucratic apparatus falls into the category of despotism in the classic typology of Montesquieu (Tocqueville 1966, 678–79, 690).

Both Hegel's and Weber's concepts of modern bureaucracy are antithetical to this line of criticism. Their common approach is to distinguish the genesis of a social formation from its function. Bureaucracy undeniably originated from the struggle between monarch and aristocrats; the crown used the efficient and merit-based bureaucratic apparatus to encroach on the privileges of aristocrats. However, having accomplished this historical role, bureaucracy be-

TABLE 1

Weber's and Hegel's Theories of Modern Political Authority

CRITERIA	LEGAL-RATIONAL AUTHORITY (WEBER)	THE POLITICAL STATE (HEGEL)
Legitimacy	legality	political sentiment
Organization		
Division of powers	constitutional division of jurisdiction	
Top positions	organs, elected or appointed political leaders, notables, or professional politicians	
Administrative staff	officials (civil servants)	
Structure	hierarchy of jurisdiction of "agencies"	
Office	defined as "impersonal rights"; separation of official and private sphere; fixed remuneration	
Criterion for recruitment	expertise	knowledge
Type of stratification	commercial classes and status groups	universal class
Mode of activity	technical application of expertise	"subsumption"
The ruled	citizens	
Relation of the partial orders	functional differentiation; state administration	
Economic basis	market economy	civil society

Note: The criteria, except "mode of activity," are adopted from Schluchter 1981, 120. Schluchter is utilized on account of his systematic arrangements. For a direct comparison between Hegel and Weber, see Jackson 1986, 148–49.

comes an institutionalized organization for dealing with public affairs. Its structural functions are independent from its historical origin.[5] Based on this methodological view, Weber constructs the ideal type of legal–rational authority with its bureaucratic apparatus in contrast to the ideal type of traditional authority, especially patrimonialism (Schluchter 1981, 118–21). Hegel's theory of bureaucracy is in the same vein (*Right*, par. 236, rem.). The administrative staff in a patrimonial regime depends on the personal will of the ruler, while bureaucracy in the modern state is autonomous from the ruler's arbitrary will and regulated by objective laws. The modern constitutional state comes into being as civil society separates from the political state and as the governing norms, as well as organizations, become impersonalized. The modern state, radically different from any traditional type of political organization, provides the foundation of the liberal, rights-based concept of politics (par. 273, rem.; Weber 1978, 642). Within the framework of rule of law (in the case of Hegel) or formal legality (in the case of Weber), the traditional concept of privileges is transformed into the modern concept of rights:

> Only within the bureaucratization of the state and of law in general can one see a definite possibility of a sharp conceptual separation of an "objective" legal order from the "subjective" rights of the individual which it guarantees. . . . These distinctions presuppose the conceptual separation of the "state," as an abstract bearer of rights of domination and the creator of legal norms, from all personal authority of individuals. *These conceptual distinctions are necessarily remote from pre-bureaucratic, especially from patrimonial and feudal, structures of authority*. (Weber 1978, 998; emphasis mine)

Notwithstanding that bureaucracy has its own shortcomings, Hegel and Weber agree that it should be treated as a new political phenomenon. Simply applying the traditional category such as despotism in this context is not appropriate.

THE MODE OF BUREAUCRATIC ACTIVITIES

Although Hegel and Weber agree about the organizational characteristics of bureaucracy and its modernity, they disagree about the nature of bureaucratic activity. For Hegel, it is based on knowledge; for Weber, it is mere technical application of expertise. This difference is even more important than their concord because it has great bearing on the current debates in social theory.

The opposition between Weber and Hegel can be traced to the Aristotelian distinction between craft (*techne*) and practical wisdom (*phronesis*). This distinction has been revitalized recently (Arendt 1959; Gadamer 1983; Habermas 1973; MacIntyre 1984; Oakeshott 1975). As recapitulated by Steven Smith, Aristotelian *techne* is "the orderly, methodical application of intelligence to some problems for the sake of gaining control over future contingencies" (1989a, 137). It is universal, instrumental, and teachable (p. 138). Practical wisdom, according to Aristotle, is "the capacity of deliberating well about what is good and advantageous for oneself regarded as typical of a mon of practical wisdom, . . . [about] what sort of things contribute to the good life in general" (*Ethics* 1140a, [1962]). Practical wisdom is experiential, intrinsic (i.e., noninstrumental), and contextual (Smith 1989a, 138–39). The pertinence of this distinction for understanding modern politics has been explored by Arendt and numerous other authors. As to its implication for the theory of bureaucracy, the dominant view is that bureaucracy is a teleological organization based on *techne*.

The dominant view is based on Weber's analysis. Weber emphasizes the definitive importance of technical expertise for bureaucracy: "The management of the office follows *general rules*, which are more or less stable, more or less exhaustive, and which can be learned. Knowledge of these rules represents a special technical expertise which the officials possess. It involves jurisprudence, administrative or business management" (Weber 1978, 958; emphasis original). Weber's specifications of the bureaucratic rules—stable, exhaustive, and learnable—indicate that they belong to the realm of *techne*.

The modality of "application" is dealt with in Weber's general theory of social action. He distinguishes four types of human action: purposive-rational or instrumental-rational (*zweckrational*); value-rational (*wertrational*); affectual; and traditional. For Weber, technical application belongs to purposive-rational action. This type of action involves calculating and choosing the appropriate means to achieve a given end. It cannot determine the end of the action except by making pragmatic judgments whether a specific end is achievable (Weber 1978, 24–26). The distinction between purposive rationality and value rationality parallels Weber's distinction between the ethic of responsibility and the ethic of conviction. As a neo-Kantian, Weber separates value from fact and adopts the position of "ethical polytheism," postulating that political values cannot be rationally determined but can only be acquired through an existential decision (1947, 152). Politicians settle political value in the battleground of political struggle. Bureaucracy, by contrast, is a "neutral agent," whose task is to execute, with technical precision and impartiality, the policies that emerge from that struggle. Politics and administration (end and means, value and fact) are separated radically in this perspective (p. 95). For Weber, the administrative activity of bureaucrats is *techne*: they can deliberate only about the means; their concerns are only expediency and technical feasibility.

Weber's interpretation of bureaucratic activity is adopted by most political theorists and social scientists, even though their philosophical backgrounds may differ widely. For example, Arendt views bureaucratization and the modern social sphere as an extended form of household management (1959, 29). Oakeshott considers bureaucracy as an administrative apparatus correlated with the idea of "lordship," according to which the modern state is an economic enterprise (1975, 268). Habermas regards bureaucratization as the main symptom of modern "scientization of politics" (1970, 62) or "mediatization of the life-world" (1984–89, vol. 2, 186). Hegel's theory of the executive power has also been interpreted in this way. Foster draws a parallel between the guardians in Plato's *Republic* and Hegel's "universal class" and suggests that the actions of both classes are *techne* (1935, 143, 182). Riedel characterizes bureaucratic activities as mere administration, thus tacitly accepting the *techne* model (1984, 152).

In my interpretation, however, Hegel's understanding of bureaucratic activity is entirely different from this dominant view. It constitutes Hegel's original contribution to a liberal theory of bureaucracy and is a creative transformation of classical political philosophy.

Hegel depicts the activity of civil officials as subsumption, a concept whose meaning needs to be scrutinized.[6] He employs this term in only two contexts in the *Philosophy of Right*: bureaucratic activity and adjudication (*Right*, pars. 225–28).[7] He specifies the operation of subsumption in detail when explaining the judiciary:

> The task of the court is the application of the law to a *single case*, and the work of judgment has *two aspects*: *first*, knowledge [*Erkenntniss*] of the nature of the case as an *immediate, single, occurrence* . . . and, in *criminal* cases, reflection to determine the *substantial*, criminal, character of the deeds; *secondly*, the subsumption of the case under the *law* so that right be restored. (par. 225; emphasis original; trans. based on Knox's).

The process of subsumption has three characteristics: it is an activity based on knowledge,[8] it mediates between universal legal norms and individual cases, and its objective is to fit norms into concrete situations so that right can be ascertained. Since Hegel draws a parallel between adjudication and execution, the question then becomes whether judicial judgment is a technical or a practical task—whether subsumption is a mere application (thus a kind of *techne*) or involves the use of practical judgement (thus a kind of *phronesis*).

Hegel takes the category of subsumption to be a familiar term in jurisprudence and does not explicate it further in his lectures (Hegel 1973–74, 1983). It is therefore difficult to determine to which type of action subsumption belongs. If compared with the dominant view of bureaucracy, however, it is notable that the realm of bureaucratic action is constituted by reference to the relation between universal norms and particular events. In other words, bureaucratic action is not merely problem solving or maximum realization of policy goals already set by politicians.

Contrary to the dominant view, hermeneutics addresses the issue of mediation between the universal and the particular from a different perspective. The philosophical dimension of the jurisprudence is formulated by Gadamer:

> What the law prescribes, what a case of a given law is, is only determined unequivocally in the eyes of a formalist who endangers life. Finding the law means thinking the case together with the law so that what is actually just or the law gets concretized. For this reason the body of precedents (the decision already laid down) is more crucial for the legal system than the universal laws in accord with which the decisions are made. This is correct

> insofar as the meaning of any universal, of any norm, is only justified and determined in and through its concretization. (1983, 82).[9]

He highlights that "concretization of norms" is the crucial element in the adjudication process. Similarly, Hegel draws a parallel between the judiciary and executive in that they have the common modality of concretization. Subsumption, like concretization or "thinking the case together with the law," is a dialectical process in which the universal and the particular encounter each other and become related by means of human deliberation. It requires a hermeneutics of the concrete, an ability to absorb sufficient contextual knowledge and relevant legal norms. It is a standard case of hermeneutic circle. The whole (norms) and part (occurrences) cannot be related except by the practical judgment of a prudent human being (*spoudaios*, in the sense of Aristotle, *Ethics* 1113a).[10] Since Gadamer proposes to reformulate Aristotelian practical philosophy through hermeneutics, his theory of hermeneutic circle is a fine reference framework for interpreting Hegel's notion of subsumption. For the sake of comparison, the competing positions suggested by Plato and Weber need to be addressed.

For practical philosophers, the Weberian dichotomy of value-rational and purposive-rational actions has shortcomings similar to those of Plato's idea of *techne*. The Platonic *techne* assumes that it is possible to realize a given scheme without changing the scheme itself in the process (Foster 1935, 18). Plato's theory is untenable because it presupposes an Archimedean point transcending the phenomenal world, by which the current order of things can be radically reformulated. The philosopher–king is the *demiourgon* (supreme craftsman) in the human world. As Arendt points out, the rule of the philosopher–king is, in fact, one-man rule in the modality of tyranny (1959, 197–203).

The Weberian politics/administration distinction does not deal adequately with bureaucratic activities either, because no set of rules could really foresee all possible occurrences. Since Weber does not admit any creativity in bureaucratic action, those decisions not stipulated by general rules can only be designated negatively as "discretion" (Weber 1978, 979). This approach results in a paradox: bureaucracy is supposed to implement political decisions with technical precision, but this ideal can never be fully realized because bureaucrats would always have some space of discretion. As a result, Weber resorts to the dubious charismatic–plebiscitary democracy to ensure the "autonomy" of political leadership; but the danger of such leadership for a liberal–constitutional regime is obvious (Mommsen 1984, 390).

Compared with the positions of Plato and Weber, practical philosophy (adopted by Aristotle, Hegel, and Gadamer) grasps political reality more adequately. For practical philosophers, there are no norms that would not themselves be modified in the process of concretization. By the same token, sociopolitical reality cannot be known apart from its normative dimension. Norms and occurrences interact continuously to form social practices (MacIntyre 1984, 187). This line of consideration, which emphasizes the immanent, instead of transcendent, nature of political action, make it easier to comprehend and appreciate the category of concretization proposed by practical philosophers. Political action is not foundational but contextual. Practices constitute the boundary of human deliberation and action. Bureaucrats, like judges, can work only under norms and use their practical judgement to arrive at the best results within the framework of laws. By placing it in the category of subsumption, Hegel indicates that bureaucratic activity is *phronesis*, not *techne*. He "judicializes" the exercise of executive power: both the judiciary and executive concretize legal norms in particular situations.[11] They are institutionalized *phronesis* in the modern state.[12]

Hegel specifies the form of practical judgment not in the *Philosophy of Right* but in the *Science of Logic*. He defines judgement as the connection between universal and particular in the concrete form of a proposition. Two aspects of his theory are relevant here. First, the attributes of the abstract universality can be recognized only through the faculty of judgment,[13] in which the universal and the particular become subject and predicate so that their relation is illuminated (*Logic*, par. 170 [1975]). Second, the attributes of the abstract universality can be specified only through a continual process of making judgment. These two aspects bear notable similarity to the category of subsumption. The second aspect is especially pertinent. It shows that judgment and subsumption are not static but are in a process of development, since the universal always has potential to realize itself in different forms while circumstances of the particular are always changing (par. 161). In a constitutional state, where norms are ultimate, institutional agents for concretizing norms are therefore indispensable. The process of subsumption coexists with the normal activities of the state.

The interpretation suggested here is supported by Hegel's discussion of the parallel bases of the judiciary and bureaucracy. Aristotle points out that since practical wisdom exercised in concrete situations depends on judgment of the agent concerning contextual circumstances, it is not possible for practical science to attain the same degree of certainty as theoretical science (*Ethics* 1094b). This inherently conditional precision poses a predicament for the judiciary and bureaucracy as institutionalized *phronesis*. If the involved parties contest this uncertainty, these institutions can no longer execute their tasks adequately. In the case of the judiciary, since judgment is necessary for the judges and jury to evaluate concrete situations, trust (*Zutrauen*) about their rectitude is the foundation of the judicial system (*Right*, par. 228).[14] The modern concept of civil rights, especially in countries with a common law judiciary, presupposes that citizens have a shared belief in the institutional arrangements for adjudication.[15]

The principle of trust has its parallel in the state, namely, *political sentiment*, or patriotism, which Hegel defines as "the consciousness that my interest, both substantive and particular, is contained and preserved in another's (i.e., in the state's) interest and end, i.e., in the other's relation to me as an individual" (*Right*, par. 268). One might conclude that Hegel is proposing authoritarianism here in linking individual ends with those of the state. However, if this theory of political sentiment is compared with his idea about trust as the underpinning of the judiciary, it is clear that political sentiment is also a form of shared belief that is essential for a political institution performing practical deliberation. Bureaucracy, like the judicial system, would collapse if the element of trust were not presented as its constituent basis. Hegel's concepts of trust and political sentiment can be best understood as the Weberian notion of legitimacy—the indispensable belief that the claims of authority has normative validity (Weber 1978, 213, 915).

An important issue arises here: How can this belief be justified or grounded theoretically? The *techne* model of bureaucracy has the seeming advantage of explaining the legitimacy of bureaucratic domination simply by technical expertise. But Weber's concept of legality is a circular argument, as Habermas has shown (1984–89, vol. 1, 265). In the interpretation proposed here, practical wisdom constitutes the crucial qualification for bureaucrats to rule. This idea is neither an elitist idea of governing nor self-consecration by the bureaucrats, as Marx insists it is (1975, 115). The ground of bureaucrats' capacity for practical judgement lies rather in the specific form of institutionalization of the modern state and the effects this form has on the officeholders. Contrary to Habermas' contention (1984–89, vol. 2, 306), the separation of public offices from their incumbents makes the cognitive orientation toward others more feasible than in any other political institutions because political resources are no longer viewed as a ruler's private property or privileges. Although every rational human being is endowed with the capacity to make moral judgment, self-interest is also a contrary compulsion with binding force. While modern economic society materialize the idea of self-interest, the modern state institutionalizes the impersonal aspect of sociopolitical relations.[16] In the modern bureaucratic organization, the officeholders must deliberate state affairs not from the egoistic perspective (prudential judgment) but from the universal-normative perspective (moral judgment). (See Manzer 1984, 579–80.) This distinction between the moral judgment of state officials and the prudential judgment of the members in civil society corresponds to the Hegelian distinction between universality and particularity and constitutes the ground of the legitimacy of modern bureaucratic rule.[17] In his theoretical sense, modern bureaucracy and judiciary could be viewed as institutionalized *phronesis*.

SOME ISSUES IN HEGEL'S POLITICAL THOUGHT

The interpretation of bureaucratic activity as *phronesis* can solve some difficulties in Hegel's peculiar theory concerning the division of constitutional powers. He opposes the concept of checks and balances (*Right*, par. 272, rem.) and proposes a constitutional division of powers into monarch, executive, and legislature. One point is noticeable: Hegel makes the judiciary part of the executive. In the interpretation advanced here, this attribution does not signify that he rejects liberalism or constitutionalism. The reason is rather that since Hegel views the two jurisdictions as endowed with the same principle, he merges them in his theory of the state.

Judges and state officials are engaged in the similar mode of activity, but they work within different domains. The universal for the judges consists of private (civil and criminal) laws, the particular of individual interactions in civil society. Judges maintain rectitude by implementing civil and criminal laws. By contrast, the universal for state officials is public laws, the particular the external effects of private pursuits in civil society: "The *maintenance* of the *state's universal interest* and of *legal norms* in this sphere of particular rights, and the work of bringing these rights back to the universal, have to be superintended by holders of the executive power." (*Right*, par. 289; emphasis original; trans. based on Knox's) Although the distinction between public and private laws is more notable in the continental legal tradition than in the common law tradition, the jurisdictions of state officials delineated by Hegel still have relevance for contemporary liberal regimes. Governmental intervention is needed for pursuing undertakings of common good (par. 235), adjusting the competing interests of producers and consumers (par. 236), and supervising education crucial for training individuals to earn an independent living (pars. 237, 239).[18] Hegel's theory of the state is close to modern welfare economics, because the functions mentioned concern matters that economists (Lindblom 1977, 78–85; North 1981, 20–44) designate as needing governmental intervention: either as externalities (contending interests of producers and consumers) or as public goods (undertakings of common good and education).

These roles indicate that for Hegel, bureaucracy is *not* a teleological organization with an externally imposed end to implement. Since bureaucrats perform their function under a framework of legal norms, bureaucratic organization is regulative (i.e., it concretizes legal norms), rather than teleological in the strong sense of being related to certain substantive goals.[19] Hegel's outline of the role of bureaucracy in the modern state should mitigate much liberal suspicion about it.[20]

The task of bureaucracy, then, is to preserve the legal norms of the state. Heretofore, the political communities had been maintained by tradition, customs, and tacit shared understanding. With the growth of modern civil society, the private sphere, and the secularization process, these traditional mechanisms become less effective. Consequently, Hegel designs an institutionalized agent that can conserve the integrity of the political community. Only through the continuous subsuming activities of bureaucracy could the constitutional state sustain itself in the face of the disruptive forces of civil society. State officials constitute a "universal class," not because they are endowed with some metaphysical knowledge of universal character like Plato's philosopher–king or Marx's proletariat but because they actualize universal constitutional norms.

Hegel's theory of bureaucratic activity, formulated by the categories of practical philosophy, is based on his evaluation of modern civil society. The quintessence of his theory is that bureaucracy is a new means of sustaining the constitution. Put in a philosophic way, bureaucracy embodies the principle of individualization of abstract legal norms in the modern constitutional state. Hegel's theory is not an anachronism, as Ilting suggests (in Pelczynski ed. 1971, 99), but a well-founded effort to overcome the destructive potential of modern subjective freedom vividly portrayed in his *Phenomenology of Spirit*. Hegel's effort is more realistic and prudent than other diagnoses of the predicaments of modernity, especially the aporia of the critique-of-instrumental-reason thesis.[21]

Hegel's liberal theory of bureaucracy overcomes the traditional dichotomy of politics and administration. Its ideal, *Rechtsstaat* (the state based on laws) is a constitutionalist concept of politics—supremacy of the principle of the rule of law. But unlike neo-Kantian liberals, Hegel endowed liberalism with a historicist, or contextual, dimension. Based on the idea of freedom realized through history, liberal constitutionalism is a politics of immanent reform of current practices (Smith 1989c). Norms and circumstances are in an unending process of mutual adjustment because present practices are continuously modified within the framework of norms. It is politics of mediation between civil society and the state (which are relatively autonomous toward each other) through the practical judgement of state officials' handling situations in which norms are challenged or in need of new interpretations. Hegel's idea of politics bears resemblance to the civil association described by Oakeshott: "The idea of *civitas*, not itself an enterprise, . . . [is] an association of *civis;* a relation of equals, and a self-sufficient condition in being relationship in terms of the conditions of a practice which are not used up in being used and not in terms of a substantive purpose or purposes to be pursued" (1975, 183). Bureaucracy in the sense specified by Hegel is *compatible* with the basic principle of constitutional liberalism. The task of modern bureaucracy —to realize the political norms in concrete situations and to subsume the latter under the universal norms—is a type of "ruling," not mere management. Moreover, for Hegel, bureaucracy is *indispensable* for the modern constitutional state. This difference between Hegel and orthodox constitutionalists like Oakeshott results from their different evaluations of modern civil society. For the latter, civil society with the rule of law is enough to form *societas*. For Hegel, in the modern constitutional state, an institutionalized agent is needed to ascertain the normative validity of constitutional norms in view of the disruptive potential of civil society. Only from this perspective can one explain why Hegel embraces modern bureaucracy, given his liberal position concerning individual rights and the rule of law.

CONCLUSION

Bureaucracy has become an indispensable governing organization in the modern state. For most liberals and constitutionalists, it may be irresistible but suspect. Hegel's theory can contribute to a liberal reinterpretation of modern bureaucracy. His ideas can be summarized into three theses. First, the rationale of bureaucracy is the division between modern economic society and the constitutional state. Second, bureaucracy is an impersonal organization in which the separation between office and its incumbent is essential. Third, the mode of bureaucratic activities is mediation between constitutional norms and concrete situations, subsuming the particular into the universal and concretizing universal norms. These three are necessary conditions of modern bureaucracy.

In organizational characterizations, Hegel's model is close to Weber's ideal type of bureaucracy but lacks Weber's antiliberal preference for plebiscitary democracy. Hegel's interpretation of bureaucratic activity as *phronesis* provides an alternative set of normative theses to the Weberian approach.[22]

Hegel's theory of bureaucracy is compatible with the liberal and constitutionalist concept of politics. In the twentieth century, bureaucracy and the scope of civil liberties have simultaneously expanded in the liberal states. This historical development illustrates that bureaucracy and liberalism do not contradict each other in essence. By contrast, the recent breakdown of socialist regimes shows that without an autonomous civil society, there is no *modern* bureaucracy, but only traditional bureaucratic empire or despotism. In both respects, Hegel's conceptualization is more appropriate than Tocqueville's or Marx's.

But Hegel is first and foremost a philosopher. His Aristotelian reconciliation between constitutionalism and bureaucracy shows that the classical political philosophy can be synthesized with the liberal rights-based problematic to gain fruitful results.

Notes

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1. This work will be cited as *Right*, with paragraph numbers. Rem. refers to Hegel's remarks; add. refers to the additions edited by Edward Gans. Hegel's emphases, omitted by Knox, are restored.

2. I thank Rogan Kersh for drawing my attention to Jackson's article.

3. It is symptomatic that no articles in Pelczynski 1971, 1984; Stepelevich and Lamb 1983; Verene 1980—all anthologies on Hegel's political philosophy—deal with his theory of bureaucracy. Bernard Cullen states explicitly that Hegel's theory of the rational state is an "unsuccessful attempt" (1979, 115).

4. The ethical dimension is viewed by Jackson as the most original aspect of Hegel's theory of bureaucracy (1986, 150–53).

5. This methodological distinction plays a prominent role in Weber's interpretation of the rise of capitalism. The innerworldly asceticism of Calvinism has an elective affinity with the capitalistic spirit of rational calculation. However, once capitalism becomes a social reality, its origin is no longer relevant to its operation.

6. Marx notices the importance of this category but insists that "Hegel does not inquire whether this mode of subsumption is adequate or rational" (1975, 109). This verdict is incorrect, as will be shown.

7. To be exact, the term *subsumption* occurs four times in the *Philosophy of Right* (pars. 225, 228, 283, 287).

8. Knox obscures the original meaning by rendering *Erkenntniss* as "ascertainment."

9. For a fuller treatment of legal hermeneutics see Gadamer 1975, 274–305.

10. There is nevertheless an important difference between Aristotle and Hegel. For Aristotle, the moral capacity of the *spoudaios* is derived from embracing the cosmic or divine order (Voegelin 1989, 68). The *spoudaios* is therefore above the laws. Aristotle's view represents the typical Greek attitude, which regards the rule of the wise person the best form of governing. For Hegel, the prudence of modern bureaucrats originates from the specific institutionalization of the modern state (separation between offices and incumbents). Due to this difference between Aristotle and Hegel, it is more to my purpose to use Gadamer's "historicized Aristotelianism." It should be noted that Gadamer's view is not without textual evidence in the Aristotelian corpus; and it is closer to Aristotle's idea of justice (*Ethics* 1134a).

11. That many executive decisions are endowed with judicial nature has been noticed by scholars of constitutional law (Friedrich 1968, 115).

12. Manzer (1984) undertakes to incorporate the Aristotelian practical judgment into the theoretical framework of policy analysis. He demonstrates that two dominant models in public policy literature—elite-planning and pluralist-exchange models—are insufficient for analyzing bureaucratic behavior. The category of practical reasoning can extend the sequential dimension and typological sophistication of the current research. Manzer highlights the judicial policymaking and commission policymaking (concerning education and welfare) as especially embodying the moral dimension of practical reasoning.

13. As Hegel (*Logic*, par. 166) points out, the etymology of the German word *Urteil* (judgment) means "original partition or distinction."

14. *Zutrauen* is rendered "confidence" in this paragraph but "trust" in par. 268. Knox thus misses the parallel drawn by Hegel.

15. Avineri has pointed out that many Hegelian concepts are developed from English practice (1972, 192).

16. Hegel's theory of the separation between civil society and the state is strongly opposed by Marx (1975, 221, 228–34). For Marx, the universality of the modern constitutional state is merely a guise for veiling exploitation. He proposes that abolishing the differentiation between society and the state is the main task for human emancipation. The historical experience of socialism shows that the separated civil society and constitutional state functions better than the unified totalitarian state, whether considered in terms of efficiency or equity. The theoretical grounds for the plurality of human associations have been tackled by Aristotle (*Politics* 1261a, 1263b [1984]) in his criticisms of the Platonic idea of a unified polis.

17. A related issue—how the belief in legitimacy is generated—is an empirical one and may vary according to specific circumstances of every state.

18. These are jurisdictions of the "police," through which the executive power functions in the civil society.

19. The term *regulative* is borrowed from Rawls (1971, 528). It means that the "larger plan" of the community should realize individual plans but that the only legitimate larger plan is justice, rather than any dominant end. The rule of law plays this regulative role in Hegel's system. Bureaucrats cannot transgress this limit. Cf. Weber 1978, 51.

20. Teleology is the underlying reason for liberals and libertarians to suspect bureaucracy. See Hayek for the distinction between "nomocratic" and "telocratic" orders (1967, 163).

21. The aporia consists in this thesis' total rejection of both modernity and the romanticist reaction. The only direction left is utopian anticipation (Benhabib 1986, 163–82).

22. They may contribute to the task proposed by Luhmann, namely, to transcend Weber's premises so as to get a critical standpoint for examining the Weberian paradigm (1982, 21).

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BUSY VOTERS, AGENDA CONTROL, AND THE POWER OF INFORMATION

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The correspondence between individual preferences and electoral outcomes is often affected by the existence of information asymmetries among electoral participants and the presence of individuals or groups who exercise some form of agenda control. While the effects of agenda control in political decision making are widely recognized, the effects of information asymmetries are not as well understood. Since information asymmetries are fundamental characteristics of most elections, a deep understanding of the correspondence between individual preferences and electoral outcomes requires a serious consideration of the "effects" of information. I develop a generalizable agenda control model that takes as given the observation that most voters are not naturally inclined to invest in political information. The model allows me to provide a dynamic description of how voters and political elites can adapt to the information problems that characterize political decision making. It also allows me to demonstrate the effect of these adaptations on electoral outcomes.

I develop a spatial election model that demonstrates the effect of information asymmetries (the fact that some electoral participants have more accurate information than others) on electoral outcomes. The original motivation for the development of this model came from the debate about the use of the initiative and referendum in the determination of public policy.[1] My intent is to advance this debate by identifying conditions under which voters are able to use these electoral institutions in order to enhance the "majority will" and conditions under which these institutions are likely to serve as effective policy-influencing tools for well-organized minority interests that obtain agenda control. In developing the model, I introduce assumptions that follow quite naturally from a study of these institutions (costly ballot access, the existence of credible endorsements, the assumption that voters know more about the status quo than a newly proposed alternative, and the assumption that voters are uncertain about the agenda-setter's preferences). In addition, the wide use of the initiative and referendum provides many opportunities for empirical tests of the model's predictions.

I define the *direct legislation environment* to be a one-shot interaction where a completely informed monopoly agenda-setter proposes one "alternative" to an existing status quo. Incompletely informed voters (direct legislators) choose one of two actions, *vote for the status quo* or *vote for the alternative*. Majority (or supermajority) rule determines a policy outcome. Both the agenda-setter and the voters have preferences over outcomes.

The binary nature of the choice offered to voters in direct legislation environments ensures that the electoral outcome is the "stated preference" of a majority of voters. However, when voters possess incomplete information about the content of the electoral alternatives, and are therefore uncertain about the relationship between their preferences and possible electoral outcomes, the relationship between the majority's "underlying preferences" and the majority's "stated preferences" may no longer be straightforward or obvious. For instance, when voters possess incomplete information, they may cast a different vote than they would have cast if they had possessed better (or complete) information. When information affects voting behavior in this way, the electoral outcome may not be same as the outcome that a better informed electorate would have chosen. Since voters are rarely, if ever, completely informed, I think that it is important to consider the use of different information conditions when approaching the study of elections.

I use the model to identify the effects of information asymmetries and communication opportunities on the correspondence between individual policy preferences and electoral outcomes in the direct legislation environment. I show conditions under which monopoly agenda control and *any* degree of voter uncertainty force direct legislation voters into having to choose between the status quo and the agenda-setter's most preferred policy. (This also implies that there exist preference profiles for which *any* policy can be reached in one vote.) Under these conditions, neither the status quo nor the setter's most preferred policy need be closely related to the median voter's (or any other voter's) preferences. I also show that the electoral winner between these two alternatives need not be the outcome that a completely informed electorate would choose.

While the lack of dependence between electoral outcomes and voter preferences, identified herein, may trouble students and supporters of democratic institutions, there is a way that a majority of voters can more effectively influence direct legislation outcomes. One of my objectives is to demonstrate that the observation, "Most voters are not naturally inclined to invest in information about politics," does not necessarily lead to the conclusion that voters are completely uninformed or that such voters will behave differently than they would have if they had more accurate information. I show conditions under which the presence of groups or individuals who are willing to undertake costly actions in an attempt to influence electoral outcomes provide incompletely informed voters with low-cost information cues. These cues allow voters to make more accurate inferences about the electoral alternatives, which increases the likelihood that they cast the same vote they would have cast if they had acquired more accurate information. In other words, I show how both self-interested voters and political elites can adapt to the information problems that characterize political decision making. Voters can adapt by using certain types of low-cost information cues to make more accurate inferences. If the agenda-setter's actions affect (do not affect) the content of the cues that will be available to voters, then the agenda-setter can adapt by giving a greater (lesser) consideration to voter preferences when choosing to take an action.

The design of the model is sufficiently generalizable to allow the results to address controversies associated with other decision-making institutions. For example, this model can provide greater insight into the actions of Denzau and Mackay's (1983) gatekeeper (a committee or an individual who has the opportunity to present a bill to a legislative body under a "closed rule") and Niskanen's (1971) bureaucrat (an individual within a bureaucratic agency who possesses better information than legislative counterparts and can offer all-or-nothing packages of bureaucratic services). I also believe that the communication apparatus incorporated into this model can be transferred to a wider range of decision-making models than I discussed.

Next, I discuss the foundations of this spatial model and establish preliminary results. I then derive electoral equilibria that allow me to demonstrate how information asymmetries and communication opportunities affect player strategies and electoral outcomes. A concluding section summarizes the findings.

FOUNDATIONS OF THE MODEL

To study the effect of information on the correspondence between individual policy preferences and electoral outcomes, I develop a model whose predominant features are *monopoly agenda control, incompletely informed voters, and communication opportunities.* Before embarking on the model's development, I briefly review the origins of the model's predominant features.

The Relation of This Model to Previous Work

The effect of introducing a monopoly agenda-setter to a spatial election model was first detailed in the "setter model" of Romer and Rosenthal (1978, 1979). Romer–Rosenthal is a complete information model where voters know the exact location of the status quo, the alternative proposed by the agenda-setter, and the agenda-setter's preferences. The agenda-setter has the opportunity to make one take-it-or-leave-it offer to the voters. In equilibrium, the setter makes an offer that maximizes setter payoff subject to the constraint that the median voter prefer the offer to the preexisting status quo. The Romer–Rosenthal model demonstrates how the presence of a monopoly agenda-setter can produce a difference between the median voter's most preferred policy outcome and the electoral outcome.[2]

The decision makers in my model are an electorate of incompletely informed voters. It is especially true in large electorates that voters who consider their opportunity costs may discover that they have little incentive to acquire detailed information (Downs 1957; Popkin 1991; Popkin et al. 1976). However, uninformed voters do not necessarily cast different votes than they would have cast if they had possessed better (or complete) information. McKelvey and Ordeshook's (1985, 1986) spatial two-candidate incomplete information models provide examples of how incompletely informed voters can use low-cost information sources to cast the same votes they would have cast if they were completely informed.[3]

The development of an incomplete information agenda control model may bring to mind the models of Denzau and Mackay (1983) and Banks (1990). In the former, the setter has incomplete information about voter preferences, and the voters are completely informed. Denzau and Mackay find equilibria for a number of different behavioral assumptions. Banks's model is more closely related to the model I am developing for it seeks to analyze the effects of voter uncertainty. A comparison of the fundamental assumptions underlying the Romer–Rosenthal, Denzau–Mackay, Banks, and my own model are presented in Table 1.

While Banks's assumptions provide the foundation for a reasonable model of some decision-making institutions, they are not as useful for the analysis of many others, like ballot initiatives or referenda. For instance, my model assumes that voters know more about the status quo than about the alternative; Banks assumes the opposite. The assumption that voters know more about the status quo is better suited for models of voting on constitutions, zoning ordinances, or taxation rules (to name a few). In these circumstances, voters must choose between status quo policies, (whose effects they might be able to observe) and alternative policies (whose effects cannot be observed until they are implemented). My model also assumes that voters can be uncertain about the agenda-setter's preferences; Banks assumes that voters know the agenda-setter's preferences. Banks's assumption requires that voters know exactly what trade-offs a setter is willing to make under any possible circumstance. The assumption that voters are uncertain about the agenda-setters preferences is much less restrictive and thus more broadly applicable to the study of political decision making. A third difference between the models is the assumptions made about communication between the agenda-setter and the voters. Signaling opportunities exist in both models, but the structure and generalizability of

TABLE 1.

Comparison of Fundamental Setter Model Assumptions

CHARACTERISTIC	ROMER AND ROSENTHAL 1978	DENZAU AND MACKAY 1983	BANKS 1990	LUPIA
Single-shot game	Yes	Yes	Yes	Yes
Single-peaked voter preferences	Yes	Yes	Yes	Yes
Perfectly informed setter	Yes	No	Yes	Yes
Voters always know				
Location of status quo	Yes	Yes	No	Yes
Setter's Preferences	Yes	Yes	Yes	No
Location of Alternative	Yes	Yes	Yes	No

the interactions are very different. Banks's voters make inferences about the status quo from the setter's choice of alternative. My voters draw information from beliefs about the setter's preferences, the observation that an election takes place, and the presence of credible endorsements. The communication opportunities in my model are more dynamic and thus more appropriate for the class of examples I am interested in.

Definition of the Model

I model the *direct legislation environment* as a one-period, multistage game of incomplete information. The object of the game is to choose one policy from a finite continuum of possible policy alternatives. One completely informed monopoly agenda-setter (*the setter*) can propose one alternative to a common knowledge status quo. The setter's willingness to propose an alternative may be affected by the cost of contesting the election. If the setter decides that proposing an alternative is not worthwhile, the game ends and the status quo is the outcome. If the setter decides to propose an alternative, voters must vote for either the status quo (about which they are completely informed) or the setter's proposed alternative (about which they possess incomplete information). Some of the setter's actions may provide additional information to voters about the alternative. All players have policy preferences, and majority (or supermajority) rule determines the outcome of the election.

In order to identify the effects of information in the direct legislation environment, I solve for equilibria in three substantively relevant game types and one control case. In the control case, the setter and the voters do not communicate. In the other game types, distinct forms of communication are available. Since the only difference between a game type and the control case is the presence of a specific form of communication, a comparison of equilibrium behavior in the control case to equilibrium behavior in the game types allows me to identify the effect of these communication opportunities on voting behavior and electoral outcomes. All players know the game type (what communication opportunities will be available) with certainty when it is their turn to choose a strategy. While the substantive motivation for each game type is provided later, the game types are briefly listed here to facilitate a compact description of the model.

Control case. No information is transmitted between potential information providers and voters.

Costly entry. Voters observe setter's decision to pay a price to contest the election.

Endorsement. Voters observe a perfectly reliable endorsement.

Learn setter's ideal point. Voters learn the setter's ideal point.

Consider the policy space [0, 1].[4] There exists a common knowledge status quo $SQ \in [0, 1]$. It is common knowledge that the game is being played by $n + 1$ players. n of the players ($N = \{1, \ldots, n\}$, where $n > 1$) are called voters and one other player is called the setter.

Each player's preferences over the policy space can be distinguished by the location of his or her ideal point. The setter's ideal point, $X \in [0, 1]$, is drawn from the cumulative distribution function F, which has density f. Each voter's ideal point, $T_i \in [0, 1]$, is drawn from the cumulative distribution function G, which has density g. All players know their own ideal points and the distribution from which any other player's ideal point is drawn. For example, the distribution F represents the prior beliefs that voters have about the setter's ideal point.[5] In short, no voter knows any other voter's ideal point, whether voters know the setter's ideal point is modeled as a variable, and the setter has complete information.[6]

After player ideal points are determined, the setter chooses a strategy. The setter's strategy, $s(X)$, has two components. The first strategic decision is whether to contest the election. In this model, the decision to contest the election is nontrivial because the setter who decides to contest will face a nonnegative, common knowledge *cost of entry*, $K \in \Re^+$. I define the *setter's entry decision* as $s_1(X) \in \{0, 1\}$, which equals 1 if the setter decides to contest the election, 0 otherwise. If the setter decides not to contest the election, the game ends and SQ is the outcome. Otherwise, the setter enters and chooses the second component of the strategy (a location for [the exact

TABLE 2.

Comparison of Endorsement Content

GAME TYPE	CONTENT	VALUE: $M_2(S_2)$ =
Control case	no endorsement	0
Costly entry	no endorsement	0
Endorsement	s_2 is to the left of SQ	−1
	s_2 is the same as SQ	0
	s_2 is to the right of SQ	1
Learn setter's ideal point	setter's ideal point	$X \in [0, 1]$

content of] the "alternative to the status quo")—$s_2(X) \in [0, 1]$.[7]

The setter's strategy set is $S_0 = \{s \mid \text{where } s\colon [0, 1] \to \{0, 1\} \times [0, 1]\}$, where the particular strategy chosen by the setter takes the form: $s(X) = (s_1(X), s_2(X))$. For notational simplicity, I denote $s(X)$ as s, $s_1(X)$ as s_1, and $s_2(X)$ as s_2. It follows that when an election is held it is of the form: SQ versus s_2.

After the setter moves, the voters choose a strategy (cast a vote). All actions taken, and all information obtained, by voters are assumed to be costless to them. A voter's strategy is a binary decision, $v_i = \{-1, 1\}$, where $v_i = -1$ represents a vote for SQ and $v_i = 1$ represents a vote for s_2.

In each game type, voters can condition their choice of strategy on a piece of information provided to them about the setter's strategy. Voters observe the message $M(s, X) = (M_1(s_1), M_2(s_2))$. The value of the message is revealed to voters after the setter chooses a strategy but before the voters vote. The first component of the message allows voters to observe whether or not the election is being contested. For all game types, $M_1(s_1) = s_1$; that is, the voters directly observe whether the setter enters or not.

The second component of the message allows voters to observe an *endorsement*. The motivation for introducing the endorsement comes from the fact that voters (collective decision makers in general) may not have an incentive to acquire costly political information. These decision makers may have a greater incentive to use reliable low-cost information cues if they are available. Examples of such cues are the use of brand names in the purchase of consumer goods, induced party loyalty in legislatures (Huber 1990), the advice or statements of opinion leaders (Berelson, Lazarsfeld, and McPhee 1954), partisan identification (Campbell et al. 1960), "biased" information providers (Calvert 1985), particular events that take place during a campaign (Popkin 1991), and "the past" (Fiorina 1981). The "endorsement" provides voters with information about the location of s_2. In the model, it is common knowledge that the endorser possesses complete information and is a perfectly reliable source of information. The content of the endorsement in each game type is specified in Table 2 (the substantive motivation for each is provided later in the text).[8]

The particular strategy chosen by voter i takes the form $v_i(T_i\ M)$. The voter's strategy sets depend on the game type and are as follows.

Game Type	*Voter Strategy*
Control case S_i:	$= \{v_i \mid \text{where } v_i\colon [0, 1] \times \{0, 1\} \to \{-1, 1\}\}$
Costly entry S_i:	$= \{v_i \mid \text{where } v_i\colon [0, 1] \times \{0, 1\} \to \{-1, 1\}\}$
Endorsement S_i:	$= \{v_i \mid \text{where } v_i\colon [0, 1] \times \{0, 1\} \times \{-1, 0, 1\} \to \{-1, 1\}\}$
Learn setter's ideal point S_i:	$= \{v_i \mid \text{where } v_i\colon [0, 1] \times \{0, 1\} \times [0, 1] \to \{-1, 1\}\}$

When an election is held, the winning outcome is determined by majority rule.[9] The outcome function is, for any $s \in \{1\} \times [0, 1]$, and $v_i \in \{-1, 1\}$,

$$o(s, v_1, \ldots, v_n) = s_2 \text{ if } \sum_{i=1}^{n} v_i > 0, \text{ and}$$

$$= SQ \text{ if } \sum_{i=1}^{n} v_i \leq 0.$$

The outcome determines the payoffs to all players.[10]

I examine the case where all players have symmetric and single-peaked utility functions.[11] I define the *voter utility function* as: $U_i(x, T_i) = -(x - T_i)$. For voters, utility is solely a function of the distance between the location of the winning policy and their ideal point. I define the *setter policy utility function* to be $U_0(x, X) = -(x - X)$. The setter's policy utility is not affected by winning or losing the election except for the influence that the electoral outcome has on the position of the winning policy. Since the setter's incentives are also affected by the cost of entry, I define the setter's *utility function* as the setter's policy utility minus the cost of contesting the election: $\phi_0(s, v_1, \ldots, v_n \mid X, T_1, \ldots, T_n) = U_0(x, X) - [K \times s_1]$.

The Definition of Equilibrium Strategies

The equilibrium concept is constructed by starting with the Bayes–Nash concept and incorporating the assumptions of the model. (The formal statement and derivation are included as Appendix A.) I first incorporate the assumption that voters always vote as if they are the pivotal voter.[12] This strategy is weakly dominant with respect to the strategies of other voters. The incorporation of this assumption transforms the equilibrium concept into a variant of the *sequential equilibrium* concept of Kreps and Wilson (1982). I also incorporate the assumption, (essentially the same made by Kreps and Wilson) that voter beliefs are consistent. In short, players in this model choose strategies to maximize expected utility. These strategies are chosen with respect to the information and strategies of the other players. In equilibrium, each player's strategy is the best response possible to the chosen strategies of others, given his or her information.

I now present two lemmas that help to characterize the setter's equilibrium strategies throughout the rest of the paper. Lemma 1 is an incentive compatibility condition that establishes that the setter contests an election if and only if the benefit from contesting the election is greater than the cost. Lemma 2 establishes an equilibrium location strategy for the setter. The proof of Lemma 2 is included as Appendix B.[13]

LEMMA 1. *The setter contests an election if and only if the benefit from contesting the election is greater than the cost.*

LEMMA 2. *A weakly dominant location strategy for the setter is* $s_2 = X$.

Lemma 2 shows that if voters are uncertain about the location of the alternative when it is time for them to vote, then a dominant strategy for the setter is to choose the (setter's) ideal point. In fact, the setter can, in equilibrium, choose any point for which $f > 0$; however, the ideal point provides the highest utility when s_2 is the electoral outcome. It follows that there exist profiles of voter preferences and beliefs that will enable *any policy in the space to be reached in a single vote.*

Contrast this finding to the agenda-setter's equilibrium location strategy in the (complete information) Romer–Rosenthal model, where the presence of completely informed voters forces the setter to propose an alternative that a majority of voters prefers to *SQ* in order to obtain a new outcome. This comparison shows how agenda-setter expectations about the level of voter information can affect the content of the electoral alternatives and suggests that the effects of information are not limited to changes in voting behavior or electoral outcomes. For notational convenience in describing equilibria, I will, henceforth, refer to the cumulative distribution function of alternatives as $F(X)$, and the corresponding density function as $f(X)$.

Evaluating the Effects of Information

Before proceeding, I would like to reintroduce the question that motivated this analysis: How does information affect electoral outcomes in the direct legislation environment? In many spatial models, the median voter's ideal point is used as a benchmark by which the correspondence between majority preferences and electoral outcomes is "measured." The generalizability of this model causes any description of the relationship between direct legislation outcomes and the median voter's ideal point to be relatively verbose. Therefore, I employ a surrogate benchmark called the complete information majority preferred alternative (CIMPA). The CIMPA is the outcome, among the (one or two) alternatives offered to voters in the direct legislation environment, that ex post provides a higher level of utility to the median voter.[14] When the setter contests an election, the CIMPA is either s_2 or *SQ*. (When voters are uncertain about s_2 then, by Lemma 2, the CIMPA is either *X* or *SQ*.) When the election is not contested, *SQ* is the only alternative and is, by definition, the CIMPA. Notice also that the "complete information *minority-preferred* alternative" is the direct legislation outcome if and only if a set of decisive voters makes the following ex post–mistaken inference:

$$\int_0^1 U_i(X, T_i)\, dF\,(X|M(s, X)) > U_i(SQ, T_i) > U_i(X, T_i);$$

that is, the *expected* utility of the setter's proposal is greater than the *known* utility from *SQ*, while, in fact, the *known* utility from *SQ* is greater than the *actual* utility a voter will receive from the true alternative. Throughout the analysis, our ability to describe the likelihood that voters make ex post mistaken inferences allows us to determine the likelihood that the CIMPA is the direct legislation outcome.

IDENTIFYING THE POWER OF INFORMATION

I now use different versions of the model to examine the effects of information asymmetries and communication opportunities on voting behavior, the setter's strategy, and electoral outcomes. To identify these effects, I first introduce a control case, a direct legislation environment where the transmission of information is not a factor and the actual interaction between the setter and the voters is negligible. All other game types will differ from the control case *only* in that specific types of communication are introduced.

In the control case the setter does not have to pay in order to contest the election, and there is no endorsement. Voters do not observe s_2 but do have prior beliefs about where it is likely to be located, $f(X)$.[15] In the control case equilibrium (the formal statement and proof of this equilibrium is included in Appendix C), the setter enters and chooses the personal ideal point as the location of s_2, while all voters condition their strategy exclusively on their prior information. The electoral outcome is determined by which of *SQ* and s_2 provide a greater expected utility to the median voter. Notice that this outcome depends only on voters' prior beliefs and *is not dependent* on the setter's actual choice of s_2.[16] Also, voters are more likely to make ex post mistaken inferences and the CIMPA is less likely to be the direct legislation outcome in the control case than in any of the other game types I introduce (holding other factors constant). Another interesting implication of this equilibrium is that the farther away *SQ* is from the median voter's ideal point, the more likely it is to lose, regardless of the actual location of s_2.

The usefulness of introducing the control case will now be revealed by comparing it to two game types that differ from it only in that a specific form of communication is introduced. The particular forms of communication I introduce are the "observation of costly setter actions" and the "existence of perfectly reliable endorsements." If it is true that any voter can observe that an election is being held, can usually observe that a campaign is being run, and is sometimes supplied with endorsements, then the low-cost information cues that costly setter action and endorsements can provide allow me to develop a useful and relevant characterization of the voting behavior of those who do not take the time to obtain political information.

In addition, the two forms of communication I introduce are collectively exhaustive of how voters in this type of model can update their prior beliefs about the alternative. These forms of communication are descendants of the many examples in the economics literature that pertain to signaling.[17] All related models of direct legislation that choose to include the important role of information will have to utilize some combination or permutation of these two information types.

The Effect of Costly Action by an Agenda-Setter

I now examine the effect of both costly setter action and the information it provides on electoral outcomes. In many public decision-making institutions, a significant effort is required to get a particular alternative considered by a decision-making body. In California, for instance, the average successful initiative proponent spent $755,000 to qualify for the November 1988 ballot (Price 1988). In that same year, as I note elsewhere, proponents and opponents of five insurance reform initiatives spent over $82 million during the campaign season (Lupia 1991). Notice that only those individuals and groups most affected by a particular issue should expect to receive a positive return from expending the resources necessary to propose and support an alternative to *SQ*. Similarly, the finite length of legislative sessions suggests that marketlike forces may determine which alternatives are brought to a vote. The fact that a particular bill is presented to any legislative committee suggests that some expenditure of resources (effort) was engaged in by the bill's sponsor(s). Knowledge about the magnitude of this expenditure could provide a signal to an incompletely informed legislator (voter) about how much the bill differs from *SQ*.

In the *costly entry* game type, the setter must decide whether or not to spend K (> 0 and common knowledge) in order to contest the election. As before, voters do not directly observe s_2 but do have beliefs about it. Because the setter need not contest the election, the fact that the setter does so conveys information to the voters. The information provided to voters by this action is that the setter believes that he or she can recover (at least) the cost of contesting the election.[18] (Notice that the *only* difference between this game type and the control case is in the value of K.) For $K > 0$, the fact that the setter contests the election, along with the voters' knowledge of the setter's single-peaked utility function, implies that s_2 is not within a well-specified neighborhood of *SQ*, since electoral outcomes near *SQ* will not provide enough extra utility to make a setter's contesting the election a profitable endeavor.

Let $\epsilon(K)$ (hereafter ϵ) be a distance on the policy continuum that is an increasing function of K.[19] ϵ determines the range of alternatives within which it will never be profitable for the setter to contest an election. Since K and the shape of the setter's utility function is known, the correspondence between K and ϵ is common knowledge, and so is the distance from *SQ* within which it is impossible for the setter to recover the cost of contesting the election. Lemma 3 shows that for setters whose ideal points are located within the "range of unprofitable alternatives" $[SQ - \epsilon, SQ + \epsilon]$, there exist no policies which, given the cost of contesting the election K, will provide the setter with a higher level of utility than costlessly accepting *SQ*.

LEMMA 3. *If the setter's ideal point is located in the range of unprofitable alternatives, the setter should not contest the election.*

When $K > 0$ and the setter contests the election, voters know that $X \notin [SQ - \epsilon, SQ + \epsilon]$, and the voters' updated beliefs will have no support on this range. When the setter enters, voters observe the message (1, 0) and use Bayes Rule to incorporate this information into their beliefs about the location of s_2. This updating leads to a revised distribution of setter ideal points $F(X|(1, 0))$, which is related to $F(X)$ in the following way:

$$f(X|(1,0)) =$$

$$\begin{cases} 0 & \text{if } \mathrm{X} \in [SQ-\epsilon, SQ+\epsilon] \\ f(X)\times\dfrac{1}{1-F(\mathrm{SQ}+\epsilon)+F(\mathrm{SQ}-\epsilon)} & \text{if } \mathrm{X} \in [0, SQ-\epsilon),(SQ+\epsilon, 1]. \end{cases}$$

A voter given a single opportunity to guess the exact location of s_2 before and after observing "costly setter entry" would more likely guess correctly after the observation. The increased likelihood is due to the fact that there are fewer possible responses after the updating, and the responses that are eliminated by the updating would have all been incorrect guesses. Therefore, I claim that after observing costly setter entry, voters can make more accurate inferences about the location of s_2.

Besides allowing voters to update their beliefs in the manner just described, the size and location of the "range of unprofitable alternatives" will determine the number of voters that are members of one of two mutually exclusive and collectively exhaustive subsets of the electorate. The members of the first subset are called *centrist voters* $\{i|T_i \in [SQ - \frac{\epsilon}{2}, SQ + \frac{\epsilon}{2}]\}$ and the members of the second subset are called *noncentrist voters* $\{i|T_i \notin [SQ - \frac{\epsilon}{2}, SQ + \frac{\epsilon}{2}]\}$. The setter (being completely informed) knows the exact number of voters in each subset. Lemma 4 tells us that *centrist voters* can infer from the setter's entry, and the

TABLE 3.

Behavior and Outcomes from Experiments Based on the Model

TYPE OF DIRECT LEGISLATION ENVIRONMENT	CONTROL CASE	COSTLY ENTRY	COMPLETE INFORMATION
consistent votes (ex post)/total votes	164/246 (67)	212/289 (73)	233/246 (95)
CIMPA was the electoral outcome	22/40 (55)	69/80 (86)	37/40 (92.5)

Note: Percentages are in parentheses. CIMPA stands for Complete Information Majority Preferred Alternative.

common knowledge, that s_2 will provide them with lower utility than will SQ; that is, a voter who has an ideal point that is sufficiently close to SQ knows that anyone who pays a large amount in an attempt to defeat SQ must be proposing an alternative that is relatively far from his ideal point.

Lemma 4. *"Vote for the status quo" is a dominant strategy for all centrist voters.*

In the equilibrium of the costly entry game type (the formal statement and proof of this equilibrium is included in Appendix C), the setter enters and chooses the personal ideal point as the location of s_2 if and only if the ideal point is not located within the range of unprofitable alternatives *and* updated beliefs will lead to a majority of voters to vote for s_2. Otherwise, she does not contest the election.[20] Centrist voters vote for SQ (and never make ex post mistaken inferences). Noncentrist voters maximize expected utility, where the expectation is conditional on their ideal point and their updated beliefs about s_2. From a computational perspective, it is interesting to note that s_2 is the electoral outcome only if the number of noncentrist voters who expect to receive a higher level of utility from s_2 than SQ make up a *majority of all voters.*

We can form several testable hypotheses about the effect of information asymmetries and signaling on the relationship between voter preferences and electoral outcomes by comparing the costly entry and control case equilibria. First, both centrist and noncentrist voters can use their observation of the agenda-setter's entry decision to make more accurate inferences about the location of s_2. Second, the introduction of this type of information should result in a greater (relative to the control case) likelihood that the CIMPA is the direct legislation outcome.[21]

I have elsewhere used a series of laboratory experiments to test whether the introduction of costly-entry-type information would (1) affect the probability that incompletely informed voters cast votes that were consistent with the votes they should have cast if they were "completely informed income maximizers" and/or (2) change the probability that the CIMPA was the direct legislation outcome (Lupia n.d.[a]). Some of these experiments were based on the control case and costly entry game types. In other experiments, all players were *completely informed.* Table 3 shows that the introduction of costly entry increased both of the aforementioned probabilities in these experiments. (Compare the control case experiments to the costly entry experiments.)

The effect of the type of information introduced in this game type has important substantive implications. To the extent that we are able to project the concept of costly entry on to a broader concept of "voter observations and beliefs about agenda-setter effort," we can use this model to better understand how voters can use commonly available, low-cost information cues in order to make decisions that are more likely to lead to the outcomes that would be chosen if the electorate had acquired more accurate information.

The Effect of Credible Endorsements

I now examine the effect of perfectly credible endorsements on voting behavior and electoral outcomes. The endorsement, $M_2(s_2) \in \{-1, 0, 1\}$, represents a costlessly verifiable, truthful opinion expressed by some individual or organization about the relative merits of SQ and s_2. In this model, it is assumed that voters know that the endorser possesses complete information and is a perfectly reliable source of information. The endorsement allows us to characterize the effect that the statements of well-known individuals, trusted friends, political parties, and opinion leaders have on individual-level voting behavior. The endorsement, as modeled here, is similar to the endorsement concept introduced by McKelvey and Ordeshook (1985). (In the description of this game type, $K = 0$ is assumed. Therefore, the only difference between this game type and the control case is the availability of the endorsement.)[22]

The content of the endorsement is either "s_2 is to the left of SQ," $M_2(s_2) = -1$; "s_2 is the same as SQ," $M_2(s_2) = 0$; or "s_2 is to the right of SQ," $M_2(s_2) = 1$. Since it is common knowledge that a perfectly credible endorsement will be provided, the setter knows that the choice of s determines which of three messages, $M(s, X) \in \{(1, -1), (1, 0), (1, 1)\}$, will be sent to voters. Voters use Bayes Rule to incorporate this information into their beliefs about the location of X. This updating leads to a revised distribution of setter ideal points, $F[X|(1, 1)]$ or $F[X|(1, -1)]$, which is related to prior beliefs $F(X)$ as follows:

If $M(s, X) = (1, 1)$, then

$$f(X|(1, 1)) = 0 \in [0, SQ)$$

$$= f(X) \times \frac{1}{1 - F(SQ)} \in (SQ, 1]$$

If $M(s, X) = (1, -1)$, then

$$f(X|(1, -1)) = F(X) \times \frac{1}{F(SQ)} \in [0, SQ)$$

$$= 0 \in (SQ, 1]$$

$$\text{and } F(X|(1, 0)) = SQ.$$

The updating in the game type allows all voters to make more accurate inferences about the location of s_2.

Besides allowing voters to update their beliefs in the manner just described, the endorsement serves to divide the electorate into two mutually exclusive and collectively exhaustive subsets. Let the voters whose ideal points are located in the range where $f(X|(1, 1)) = 0$ or $f(X|(1, -1)) = 0$ be called *opposite voters* ($\{i|T_i \in [0, SQ)$, if $M_2(s_2) = 1\}$, $\{i|T_i \in (SQ, 1]$, if $M_2(s_2) = -1\}$). Let all other voters be known as *nonopposite* voters. The setter, being completely informed, knows the exact number of voters in each subset. Lemma 5 tells us that *opposite voters* can infer from the endorsement and the common knowledge that X will provide them will lower utility than will SQ. For instance, a voter who knows that SQ is to the right of his or her ideal point, and whom an endorsement tells that s_2 is to the right of SQ, then the voter knows that SQ will provide higher utility than the realization of s_2.

LEMMA 5. *"Vote for the status quo" is a dominant strategy for all opposite voters.*

In the equilibrium of the *endorsement* game type (the formal statement and proof of this equilibrium is included in Appendix C), the setter always enters and chooses the personal ideal point as the location of s_2. Opposite voters vote for SQ and never make ex post mistaken inferences. Nonopposite voters maximize expected utility, where the expectation is conditional on their ideal points and their beliefs about s_2—which themselves are conditional on the endorsement. In general, all voters can use the endorsement to make more accurate inferences, which results in a greater likelihood (relative to the control case) that the CIMPA is the electoral outcome. From a computational perspective, s_2 is the electoral outcome only if the number of nonopposite voters whose updated expected level of utility from s_2 is higher than the certain level of utility that SQ provides make up a *majority of all voters*. In addition, if the setter's ideal point is on the opposite side of SQ from the median voter's ideal point, then the presence of a credible endorser prevents the setter from obtaining an outcome preferred to SQ.

When credible endorsements are available, the model suggests that the ability to answer questions about issue specifics is not a necessary condition for casting an "informed" vote. I have elsewhere attempted an empirical verification of this suggestion (Lupia 1991). In November 1988, I conducted an exit poll that was designed to elicit a measurement of what insurance reform voters in California knew about issue specifics, the "underlying" policy preferences of prominent endorsing groups, and the explicit preferences of the prominent endorsing groups over the alternatives that qualified for the ballot. One finding from the analysis of the poll is that respondents who possessed information about endorsers but could not answer questions about issue specifics cast votes very similar to those respondents who had both similar, relevant, personal characteristics and possessed enough information to answer issue-specific questions. This finding supports my model's claim that voters can use endorsements to make more accurate inferences (i.e., the types of inferences that "informed" respondents make) and increase the likelihood that they cast the same vote they would have cast if they had possessed better, or complete, information.

The effect of introducing costly setter action and perfectly credible endorsements suggest that the voters' access to simple and publicly available sources of information can help direct legislation voters make ex post more accurate inferences. The fact that we observe political elites making efforts to provide these types of cues suggests that these actors recognize and adapt to the information problems that are inherent in political decision making. If obtaining these forms of information requires less effort than obtaining information about the true location of an alternative, then the comparative statics suggest that when credible endorsements are available, a voter can cast an "informed" vote while possessing what might appear to be very limited information.

The Effect of Learning the Setter's Ideal Point

Finally, I consider the case where voters learn the location of the setter's ideal point. I include this game type to show that the setter's equilibrium location strategy, $s_2 = X$, holds even when voters are completely informed about the agenda-setter's preferences. In this game type, the perfectly reliable endorsement tells voters the location of the setter's ideal point, X, as opposed to the directional information it provided in the previous game type. That voters know X and do not observe s_2 when it is time for them to vote yields a game that is similar to the *one seller and sophisticated buyer* model of Milgrom and Roberts (1986).

In the equilibrium to the *learn setter's ideal point* game type, (the formal statement and proof of this equilibrium is included in Appendix C), the setter always enters and chooses $s_2 = X$. Voters never make ex post mistaken inferences because their knowledge of X allows them to infer precisely where s_2 will be. The electoral result is the setter's ideal point only when the median voter prefers X to SQ. In addition, the CIMPA is always the electoral outcome.

The analysis of this game type is interesting since it provides an example of how even the smallest degree of voter uncertainty about the location of the alternative does not provide the setter with a sufficient incentive to consider voter preferences when making a location decision; that is, unless voters have full information in the direct legislation environment, the setter should choose his or her ideal point as the alternative to SQ. Intuition suggests that the voter's location strategy may be more responsive to voter preferences if either repeated play or increased electoral competition provided voters with opportunities to punish setters who propose "extreme" alternatives. The accuracy of this suggestion should be evaluated in future research.

CONCLUSION

This spatial model allows us to obtain a better understanding of the effect that different types of information have on political behavior and electoral outcomes. I use the model to provide a relatively dynamic description of how voters can use information cues to make more accurate inferences about electoral alternatives. More accurate voter inferences increase the probability that an incompletely informed voter casts the same vote he or she would have cast with better or complete information. When information cues increase the likelihood that incompletely informed voters emulate the behavior of informed voters, the probability that the complete information majority preferred alternative (CIMPA) is the direct legislation outcome increases.

I also use the model to show conditions under which information and communication opportunities affect the strategies of a monopoly agenda-setter. The most noteworthy characteristic of agenda-setter behavior in the model is that the agenda-setter should offer the personal ideal point as an alternative to the status quo when knowing that voters will be uncertain about the location (content) of the alternative. The fact that the content-determining component of the setter's strategy can be totally unresponsive to voter preferences demonstrates that the effects of political information are not necessarily limited to voter behavior or electoral outcomes. A comparison of setter behavior under different information conditions shows how the information that political elites have about the sources and content of election-day voter information might affect their preelection decisions about how to choose issues or frame campaign messages.

I now suggest two ways to enhance the applicability of this research and provide one piece of advice for those who wish for better informed voters. One direction for future research is to incorporate competition among political elites into this type of model. The presence of incompletely informed voters generates the potential effectiveness of information cues. If political elites expect cues to have an effect on voting behavior, we should not be surprised when they incorporate the provision of information cues into their electoral strategies. I expect that competition among potential agenda controllers and competition among information providers will affect the type and content of information available to voters. If competition affects information and information affects voting behavior, then research that incorporates both information and competition promises to teach us a great deal about political decision making.

I also believe that incorporating the notion of credibility into the study of political decision making is important. In most studies of politics, the role of information cues is ignored. In this model, information cues are perfectly credible. In reality, there exist many types of information cues; and they are often not perfectly credible. Those who have the resources to provide political information sometimes have an incentive to mislead voters. This possibility suggests that the credibility of the information provider could determine how certain types of information affect strategies and electoral outcomes. Credibility is a dynamic phenomenon and a complete description of its effects requires a distinct analysis. In a model related to the present one I feature credibility as a fundamental (and continuous) parameter for the analysis of elections; I find conditions under which an uninformed voter can use aspects of interaction with a possibly unreliable information provider to increase the likelihood of making the same decision the voter would have made if completely informed; and I show that the central results, derived herein, are robust to the introduction of credibility as a parameter (Lupia n.d.[b]).

I close by restating that my original motivation for the development of this model came from the debate about the use of the initiative and referendum in the determination of public policy. One of the criticisms of these institutions is that voters are not informed enough to make complex policy decisions. While educating voters about the intricacies of policy is one way to quell this type of criticism, I assert that many voters would not volunteer for such an education, since they have other things they want to do. Therefore, suppose that we can identify a class of situations where the existence of credible cues would help voters make more accurate inferences and that anticipation of the availability of these cues gives political elites greater incentives to be responsive to voter preferences. Then, those who want "more informed outcomes" might be better off ensuring that credible cues are provided to individuals who have an interest in electoral outcomes, the capacity for simple reasoning, and other things to do.

APPENDIX A: DERIVATION OF THE EQUILIBRIUM CONCEPT

I define the equilibrium by stating the Bayes–Nash equilibrium for this game and then incorporating the assumptions of this model into the Bayes–Nash statement. Let $\bar{M}$ denote the message space which is $\{0, 1\}$ times the space defined in Table 2.

$\forall (k, j) \in \bar{M}$, let $A(k, j) = \{X: s_1 = k$, and $M_2(s_2) = j\}$ be the set of all setter types that send message $M(s, X) = (k, j)$.

In a Bayes–Nash equilibrium, $\forall X \in [0, 1]$, the setter chooses $s \in \{0, 1\} \times [0, 1]$ to maximize

$$\phi_0(s, v_1, \ldots, v_n | X, T_1, \ldots, T_n),$$

and each voter i $\in N$ and $T_i \in [0, 1]$ chooses $v_i(T_i)$ to maximize

$$\int U_i[o(s_2, v_i(T_i, M(s, X)), v_{-i}(T_{-i}, M(s, X))), T_i] \; dF(X)\, dG(T_{-i}).$$

The first difference between our equilibrium concept and Bayes–Nash is that we assume that voters always vote as if they are the pivotal voters (i.e., they adopt strategies that are weakly dominant with respect to the strategies of other voters); that is, $v_i^* \in S_i$ is weakly dominant if

$$\forall\, v_i \in S_i,\ v_{-i} \in S_{-i},\ s \in S_0,\ T_i \in [0, 1], \text{ and } T_{-i} \in [0, 1]^{n-1}$$

$$\int U_i[o(s_2, v_i(T_i, M(s, X)), v_{-i}(T_{-i}, M(s, X))), T_i] \geq \int U_i[o(s_2, v_i(T_i, M(s, X)), v_{-i}(T_{-i}, M(s, X))), T_i],$$

with strict inequality for some T_i, T_{-i}, v_{-i}, and s. If v_i^* is dominant, it must be the case that

$$\int U_i(o(s_2, v_i(T_i, M(s, X)), v_{-i}(T_{-i}, M(s, X))), T_i) \; dF(X) \geq \int U_i(o(s_2, v_i(T_i, M(s, X)), v_{-i}(T_{-i}, M(s, X))), T_i) \; dF(X),$$

with sometimes strict inequality, which implies

$$\sum_{(j,k) \in M} \int_{A(j,k)} U_i(o(s_2, v_i(T_i, j, k), v_{-i}(T_{-i}, j, k)), T_i) \; dF(X) \geq \sum_{(j,k) \in \bar{M}} \int_{A(j,k)} U_i(o(s_2, v_i(T_i, j, k), v_{-i}(T_{-i}, j, k)), T_i) \; dF(X).$$

Note that for all $v_{-i} \in S_{-i}$ and $T_{-i} \in [0, 1]^{n-1}$, i can only affect the outcome of the election if

$$\sum_{i' \in (N-i)} v_{i'}(T_{i'}, (j, k)) \in \{0, 1\}.$$

In this case

$$o(s, v_i(T_i, M(j, k), v_{-i}(T_i, M(j, k)))) = \begin{cases} s_2 & \text{if } v_i = 1 \\ SQ & \text{if } v_i = -1. \end{cases}$$

Hence

$$v_i = \begin{array}{l} 1 \text{ if } \int_{A(j,k)} U_i(s_2, T_i)\, dF(X) > \int_{A(j,k)} U_i(SQ), T_i)\, dF(X) \\ -1 \text{ if } \int_{A(j,k)} U_i(s_2, T_i)\, dF(X) \leq \int_{A(j,k)} U_i(SQ, T_i)\, dF(X) \end{array}$$

is a dominant strategy for voter i. But v_i can be rewritten as

$$v_i = \begin{cases} 1 \text{ if } \int_{A(j,k)} U_i(s, T_i)\, dF(X) > U_i(SQ, T_i) \int_{A(j,k)} dF(X) \\ -1 \text{ if } \int_{A(j,k)} U_i(s, T_i)\, dF(X) \leq U_i(SQ, T_i) \int_{A(j,k)} dF(X) \end{cases}$$

$$v_i = \begin{cases} 1 \text{ if } \int_0^1 U_i(s, T_i)\, dF(X|j, k) > U_i(SQ, T_i) \\ -1 \text{ if } \int_0^1 U_i(s, T_i)\, dF(X|j, k) \leq U_i(SQ, T_i), \end{cases}$$

where

$$f(X|k, j)) = \begin{cases} \dfrac{f(x)}{pr(k, j)} & \text{if } x \in A(k, j) \\ 0 & \text{otherwise.} \end{cases}$$

I can therefore restate the equilibrium concept for the direct legislation model (which is now more similar to the sequential equilibrium concept of Kreps and Wilson 1982 than Bayes–Nash) as a set of strategies $s \in S_0$, $v_i \in S_i$, and voter beliefs $f(X|k, j)$, such that for each $(k, j) \in \bar{M}$,

SETTER $\forall X$, $s = (s_1, s_2)$ satisfies $\max_{s \in \{0,1\} \times [0,1]}$

$$[U_0(o(s_2, v_1(T_1, M(s, X)), \ldots, v_n(T_n, M(s, X)), X)) - (K \times s_1)].$$

VOTERS $\forall T_i$, $(i \in N)$, and $\forall (k, j) \in \bar{M}$, $v_i(T_i, k, j)$ satisfies

$$v_i = 1 \text{ if } \int_0^1 U_i(s_2, T_i)\, dF(X|k, j) > U_i(SQ, T_i)$$

$v_i = -1$ otherwise.

Beliefs $\forall (k, j) \in \bar{M} : f(X|k, j) = \begin{cases} \dfrac{f(x)}{pr(k, j)} & \text{if } x \in A(k, j) \\ 0 & \text{otherwise,} \end{cases}$

where

$$pr(k, j) = \int_{A(k,j)} F(x)\, dx.$$

If we assume that the setter is completely informed, we can redefine and simplify the setter's optimization problem in the following manner. In a Bayes–Nash equilibrium, $\forall X \in [0, 1]$, the setter chooses $s \in \{0, 1\} \times [0, 1]$ to maximize

$$U_0(o(s_2, v_1(T_1, M(s, X)), \ldots, v_n(T_n, M(s, X)), X)) - (K \times s_1).$$

Now, $\forall M(s, X) \in \bar{M}$ and $s \in S_0$, let

$$C(SQ, M(s, X)) = \{|T_i|: U_i(s_2, T_i)\, dF\, (X|M(s, X)) \leq U_i(SQ, T_i)\},$$

and

$$C(s_2, M(s, X)) = N - C(SQ, M(s, X)).$$

$C[SQ, M(s, X)]$ is a count of the number of voters who receive expected utility from the SQ that is greater than or equal to the expected utility from the lottery of setter types sending message $M(s, X)$. $C[s_2, M(s, X)]$ is a count of the number of voters who receive higher expected utility from the lottery of setter types sending message $M(s, X)$ than from SQ. I can, for any $X \in [0, 1]$, redefine the setter's optimization problem as follows. *Choose* $s = (s_1, s_2)$ *to maximize*

$$\Phi_0(s, M(s, X)|X) = \begin{cases} U_0(SQ, X) - (K \times s_1) & \text{if } C(SQ, M(s, X)) \geq \dfrac{N}{2} \\ U_0(s_2, X) - (K \times s_1) & \text{if } C(SQ, M(s, X)) < \dfrac{N}{2}. \end{cases}$$

Thus, the equilibrium concept I have used is defined as a set of strategies, $s \in S_0$, $v_i \in S_i$, and voter beliefs, $f(X|k, j)$, such that for each $(k, j) \in \bar{M}$

Setter $\forall X$, $s = (s_1, s_2)$ satisfies $\max_{s \in \{0,1\} \times [0,1]}$

$$\Phi_0(s, M(s, X)|X) = \begin{cases} U_0(SQ, X) - (K \times s_1) & \text{if } C(SQ, M(s, X)) \geq \dfrac{N}{2} \\ U_0(s_2, X) - (K \times s_1) & \text{if } C(SQ, M(s, X)) < \dfrac{N}{2}. \end{cases}$$

Voters $\forall T_i$, $(i \in N)$, and $\forall (k, j) \in \bar{M}$, $v_i(T_i, k, j)$ satisfies

$$v_i = 1 \text{ if } \int_0^1 U_i(s_2, T_i)\, dF\, (X|k, j) > U_i(SQ, T_i)$$

$v_i = -1$ otherwise.

Beliefs $\forall (k, j) \in \bar{M} : f(X|k, j)$

$$= \begin{cases} \dfrac{f(x)}{pr(k, j)} & \text{if } x \in A(k, j) \\ 0 & \text{otherwise,} \end{cases}$$

where

$$pr(k, j) = \int_{A(k,j)} F(x)\, dx.$$

APPENDIX B: PROOF OF LEMMA 2

Proof. The setter's choice of strategy relays a message to voters. Voters condition their choice on the message $M(s, X) = (s_1, M_2(s_2))$. I now establish the dominance of the strategy $s_2 = X$ for the four game types I introduce.

1. From the definition of the control case and Lemma 1, the setter sends only one type of message, $M(s, X) = (1, 0)$. (The setter always [costlessly] enters and there is no endorsement.) In this case, where $K = 0$, the setter chooses s to maximize

$$\Phi_0(s, M(s, X)|X) = \Phi_0((1, s_2), (1, 0)|X)$$

$$= \begin{cases} U_0(SQ, X) & \text{if } C(SQ, 1, 0) \geq \dfrac{N}{2} \\ U_0(s_2, X) & \text{if } C(SQ, 1, 0) < \dfrac{N}{2} \end{cases}$$

If $C(SQ, 1, 0) \geq N/2$, then $\Phi_0((1, s_2), (1, 0)|X) = U_0(SQ, X)$, independent of s_2. If $C(SQ, 1, 0) < N/2$, then $\Phi_0((1, s_2), (1, 0)|X)$ is maximized when $s_2 = X$. Therefore, since U_0 is maximized at $s_2 = X$ independent of the value of $C(SQ, 1, 0)$, this strategy is weakly dominant for the setter in the control case game.

2. In the *costly entry* case, the setter can send one of two messages, $M(s, X) \in \{(0, 0), (1, 0)\}$. (The setter either enters or does not. There is no endorsement.) If $M(s, X) = (1, 0)$, then $\Phi_0((1, s_2), (1, 0)|X)$ is maximized by $s_2 = X$, as was shown in the control case section of this proof. Note that the value of $\Phi_0((0, s_2), (0, 0)|X)$, the setter's utility when the election is not contested, is not affected by s_2: $\Phi_0((0, s_2), (0, 0)|X) = U_0(SQ, X) - (K \times 0)$. Therefore, $s_2 = X$ is weakly dominant for the setter in the costly entry case.

3. From the definition of the *endorsement* game type and Lemma 1, the setter can send one of three messages, $M(s, X) \in \{(1, -1), (1, 0), (1, 1)\}$. (The setter always [costlessly] enters and there is an endorsement.) The setter chooses s to maximize (where $K = 0$)

$$\Phi_0(s, M(s, X)|X)$$
$$= \begin{cases} \Phi_0((1, s_2), (1, 1)|X) & \text{if } SQ < s_2 \\ \Phi_0((1, s_2), (1, 0)|X) & \text{if } SQ = s_2 \\ \Phi_0((1, s_2), (1, -1)|X) & \text{if } s_2 < SQ, \end{cases}$$

where

$$\Phi_0[(1, s_2), (1, 1)|X]$$
$$= \begin{cases} U_0(SQ, X) & \text{if } C(SQ, 1, 1) \geq \frac{N}{2} \\ U_0(s_2, X) & \text{if } C(SQ, 1, 1) < \frac{N}{2} \end{cases}$$

$$\Phi_0[(1, s_2), (1, 0)|X] = U_0(SQ, X)$$

$$\Phi_0[(1, s_2), (1, -1)|X]$$
$$= \begin{cases} U_0(SQ, X) & \text{if } C(SQ, 1, 1) \geq \frac{N}{2} \\ U_0(s_2, X) & \text{if } C(SQ, 1, -1) < \frac{N}{2}. \end{cases}$$

If $X = SQ$, then the proof is obvious. If $X \neq SQ$, then it remains to show that $s_2 = X$ is weakly dominant. Consider the following two subcases:

a. $s_2' \neq X$ and $s_2 = X$, where $s' = (s_1, s_2')$ and $s = (s_1, X)$, and $M(s', X) = M(s, X)$. If $s_2' \neq X$ then $U_0(X, X) > U_0(s_2', X)$ which implies $\Phi_0(s, M(s, X)|X) \geq \Phi_0(s', M(s_0', X)|X)$. (I.e., since $M_2(s_2 = X) = M_2(s_2 \neq X)$, voters cannot differentiate amongst the setter types that would send each message.) Thus, $s_2 = X$ is a weakly dominant setter location strategy in the endorsement game type when $M_2(s_2) = M_2(s_2)$.
b. $s_2 \neq X$ and $M_2(s_2) \neq M_2(X)$. Thus, $s_2 \neq SQ$. Let $s_2'(X) = (s_2 + SQ)/2$. $M_2(s_2) \neq M_2(X)$ implies that X and s_2 are on opposite sides of SQ. Thus, any $s_2'(X)$ that is closer to SQ provides a higher level of utility to the setter: $U_0[s_2'(X), X] > U_0(s_2, X)$. Hence, changing from s_2 to $s_2'(X)$ gives the setter a higher level of utility. Thus, s_2 cannot be an equilibrium partial strategy.

From Case A and Case B, I have established that $s_2 = X$ is a weakly dominant location strategy for the setter in the "Endorsement" game type.

4. In the *learn setter's ideal point* game type, the definition of the endorsement changes. $M_2(s_2) = X$ is the endorsement; $M = \{0, 1\} \times [0, 1]$ is the message space; and $K = 0$.[23] Then, I can define the setter's objective function for this game type as

$$\Phi_0'(s, M(s, X)|X)$$
$$= \begin{cases} U_0(SQ, X) & \text{if } C[SQ, M(s, X)] \geq \frac{N}{2} \\ U_0(s_2, X) & \text{if } C[SQ, M(s, X)] < \frac{N}{2}. \end{cases}$$

The setter of type X can send one of two messages in this game type, $M' \in \{(0, X), (1, X)\}$. The setter's chooses s to maximize $\Phi_0'(s, M(s, X)|X)$, where

$$\Phi_0'[(0, s_2), (0, X)|X] = U_0(SQ, X).$$

$$\Phi_0'[(1, s_2), (1, X)|X]$$
$$= \begin{cases} U_0(SQ, X) & \text{if } C(SQ, 1, X) \geq \frac{N}{2} \\ U_0(s_2, X) & \text{if } C(SQ, 1, X) < \frac{N}{2}. \end{cases}$$

Note, first, that the value of $\Phi_0'((0, s_2), (0, X)|X)$ is not affected by the setter's choice of s_2. If $M(s, X) = (1, X)$, then $\Phi_0((1, X)|X)$ is maximized by $s_2 = X$, as was shown in control case section of this proof. (The only difference between this objective function and that objective function is a constant.) Thus, $s_2 = X$ is a weakly dominant location strategy for the learn setter's ideal point game type.

APPENDIX C: FORMAL STATEMENT OF EQUILIBRIA

Control Case

PROPOSITION. *The equilibrium for the control case is*

$$\forall\ X \in [0, 1]\colon s = (1, X)$$

$$\forall\ T_i \in [0, 1]\colon v_i(T_i, M(s, X))$$

$$= \begin{cases} 1 \ \text{if} \int 0_0^1 [U_i(X, T_i)f(X|M(s, X))] \\ \qquad\qquad > U_i(SQ, T_i) \text{ and } M_1(s_1) = 1 \\ -1 \quad \text{otherwise.} \end{cases}$$

Beliefs can be characterized by $f(X|M(s, X)) = f(X)$.

Proof. The setter's equilibrium entry strategy is proven by Lemma 1. The setter's location strategy is proven by Lemma 2.

Costly Entry Game Type

PROPOSITION. *The equilibrium for the costly entry game type is*

$$\forall\ X \in [SQ - \epsilon, SQ + \epsilon]\colon s_1 = 0$$

$$\forall\ X \in [0, SQ - \epsilon), (SQ + \epsilon, 1]\colon s_1$$

$$= \begin{cases} 1 & \text{if } C(SQ, M(s, X)) < \frac{N}{2} \\ 0 & \text{otherwise} \end{cases}$$

$$\forall\ X \in [0, 1]\colon s_2 = X$$

$$\forall\ T_i \in [0, SQ - \tfrac{\epsilon}{2}), (SQ + \tfrac{\epsilon}{2}, 1]\colon$$

$$v_i(T_i, M(s, X)) =$$

$$1 \quad if \int_0^1 [U_i(X, T_i)f(X|1, 0))] > U_i(SQ, T_i) \; and \; M_1(s_1)$$

$$= 1 - 1 \quad otherwise$$

Beliefs can be characterized by $f(X|(1, 0))$ or $f(X|(0, 0)) = SQ$, depending on the value of s_1.

Proof. The setter's equilibrium entry strategy is proven by Lemma 1 and Lemma 3. The setter's location strategy is proven by Lemma 2. Centrist voter equilibrium strategies can be characterized by Lemma 4.

Endorsement Game Type

Proposition. *The equilibrium for the endorsement game type is*

$$\forall \; X \in [0, 1]: s_1 = 1$$

$$\forall \; X \in [0, 1]: s_2 = X$$

$$\forall \; T_i \in [0, SQ): v_i[T_i, M(s, X)] =$$

$$\begin{cases} 1 \;\; if \; M_2(s_2) = -1 \; and \displaystyle\int_0^1 [U_i(X, T_i)f(X|(1, -1))] \\ \qquad\qquad > U_i(SQ, T_i) \; and \; M_1(s_1) = 1 \\ -1 \;\; otherwise \end{cases}$$

$$\forall \; T_i \in (SQ, 1]: v_i[T_i, M(s, X)] =$$

$$\begin{cases} 1 \;\; if \, M_2(s_2) = 1 \, and \displaystyle\int_0^1 [U_i(X, T_i)f(X|(1, 1))] \\ \qquad\qquad > U_i(SQ, T_i) \, and \, M_1(s_1) = 1 \\ -1 \;\; otherwise. \end{cases}$$

Beliefs can be characterized by $f(X|(1, -1))$ or $f(X|(1, 1))$, depending on the value of $M_2(s_2)$.

Proof. The setter's equilibrium entry strategy is proven by Lemma 1. The setter's location strategy is proven by Lemma 2. The opposite voter equilibrium strategies can be characterized by Lemma 5.

Learn Setter's Ideal Point Game Type

Let $\tilde{M} = \{0, 1\} \times [0, 1]$ be the message space. $M_2(s_2) = X$ is the endorsement, which reveals the setter's ideal point to voters (cf. Milgrom and Roberts 1986, prop. 1).

Proposition. *The equilibrium for the "Learn Setter's Ideal Point" game type is:*

$$\forall \; X \in [0, 1]: s_1 = 1 \; if \; C(SQ, M(s, X)) < \frac{N}{2}$$

$$\forall \; X \in [0, 1]: s_1 = 0 \; otherwise$$

$$\forall \; X \in [0, 1]: s_2 = X$$

$$\forall \; T_i \in [0, 1]: v_i[T_i, M(s, X)]$$

$$= \begin{cases} 1 \;\; if \; U_i(X, T_i) > U_i(SQ, T_i) \; and \; M_1(s_1) = 1 \\ -1 \;\; if \; U_i(X, T_i) < U_i(SQ, T_i). \end{cases}$$

Beliefs can be characterized by X.

Proof. The setter's equilibrium entry strategy is proven by Lemma 1. The setter's location strategy is proven by Lemma 2.

Notes

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1. The distinction between these two election types is that referenda originate from within the government and ballot initiatives originate from outside the government. The United State is one of the few democracies that does not use the initiative or referendum to determine national level policy, but their use is extensive at the state and local level. Recent important—but largely atheoretical—contributions to this debate include those of Magleby (1984) and Cronin (1989).

2. The setter model has been examined under different assumptions about the nature of incomplete information. In Romer and Rosenthal 1979, the setter model is examined under conditions of uncertain turnout. In Morton 1988, voters condition their actions in the present period on anticipated setter actions in future periods. Rosenthal (1990) discusses other elaborations of the "setter model."

3. The idea that the presence of low-cost information cues can influence policy outcomes is powerfully made by McCubbins and Schwartz (1984). They argue that Congress can efficiently narrow the divergence between legislative goals and bureaucratic actions by designing institutions that provide interested parties with incentives to obtain information (referred to as "fire alarms") about the correspondence between legislative goals and bureaucratic activities.

4. For simplicity, the one-dimensional case is discussed. Since there are, at most, two electoral alternatives in this model, the extension of the model to higher levels of dimensionality (making standard assumptions about voter preferences) is straightforward.

5. This representation of setter preferences allows a general representation of player prior beliefs. For example, when F is uniformly distributed, voters will assume that all possible setter ideal points can be drawn with equal probability. Alternatively, when F has all of its mass on one point, the voter's priors are completely informative.

6. A simple way to relax the assumption that the setter is completely informed is to allow N to be either infinite (or large and finite, a good approximation) and let the setter's information about the voter ideal points be the distribution G. A derivation of the model using the latter assumption is available upon request.

7. For notational convenience, I denote $s_2(X) = SQ$, when $s_1(X) = 0$ (i.e., the setter chooses to accept SQ).

8. Also, since $F(X)$ and $[0, 1]$ are continuous, $SQ = s_2$ occurs with near zero probability. If this event does occur, it follows trivially that SQ is the direct legislation outcome.

9. Supermajorities follow straightforwardly by changing the values of v_i. None of my results depend on the use of simple majority rule.

10. I have assumed that the SQ wins ties. This assumption is consistent with the tie-breaking rule used in many of direct legislation environments. Changing this assumption does not

alter the power of the results that follow, but it will influence boundary conditions in straightforward ways.

11. This restriction is made for notational and expositional simplicity. The assumptions of symmetry and linearity are not necessary for the proofs of any of my lemmas or propositions.

12. This assumption is common to most spatial election models.

13. The proofs of other lemmas follow straightforwardly from the assumptions of the model and are available upon request.

14. This statement seems to depend on the nonexistence of a preference profile that will sustain an enforceable system of side payments. In the one-dimensional case, the CIMPA always exists (and, in fact, is always the core or in the core).

15. In this game type, voters always receive the same message $M(s, X) = (1, 0)$, because (from Lemmas 1 and 2) we know that when $K = 0$, contesting the election is a weakly dominant strategy for the setter and there is no endorsement.

16. Except for "opposite" and "centrist" voters, the strategies of voters can be fully characterized using a single cut-point. (This concept is used in Alesina and Rosenthal 1989.) A proof of the uniqueness of the cut-point in this model is available upon request.

17. Spence 1973 is the seminal article on signaling; Milgrom and Roberts 1986 is also directly relevant.

18. By introducing the costly entry concept, I am not attempting to make any statement about the welfare properties associated with altering the magnitude of the barriers to ballot access. I introduce costly entry, since it is a type of information that is, to some extent, observable and is sometimes (as is the case with intense legislative wrangling or initiative campaign expenditures) widely publicized.

19. $d\epsilon/dK$ and $d^2\epsilon/dK^2$ are positive; $\epsilon(0) = 0$.

20. This implies that either all setter types whose ideal points lie outside of the "range of unprofitable alternatives" choose to contest the election or none do.

21. In addition, the introduction of costly entry increases the probability that SQ is the direct legislation outcome by narrowing the likelihood that a setter will challenge it. This type of result has been produced in a direct-legislation-type experimental environment by Herzberg and Wilson (1990).

22. The characteristics of the equilibrium of a game type that included both costly entry and endorsements follow straightforwardly from the equilibria presented.

23. The information provided by voters by $K > 0$ in the previous game types is superfluous in this game type.

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COVENANTS WITH AND WITHOUT A SWORD: SELF-GOVERNANCE IS POSSIBLE

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Contemporary political theory often assumes that individuals cannot make credible commitments where substantial temptations exist to break them unless such commitments are enforced by an external *agent. One such situation may occur in relation to* common pool resources, *which are natural or man-made resources whose yield is subtractable and whose exclusion is nontrivial (but not necessarily impossible). Examples include fisheries, forests, grazing ranges, irrigation systems, and groundwater basins. Empirical evidence, however, suggests that appropriators in common pool resources develop credible commitments in many cases without relying on external authorities. We present findings from a series of experiments exploring (1) covenants alone (both one-shot and repeated communication opportunities); (2) swords alone (repeated opportunities to sanction each other); and (3) covenants combined with an internal sword (one-shot communication followed by repeated opportunities to sanction each other).*

> And Covenants, without the sword, are but words, and of no strength to secure a man at all.
>
> —Hobbes, *Leviathan*

Contemporary political theories frequently presume that individuals cannot make credible ex ante commitments where substantial ex post temptations exist to break them unless such commitments are enforced by an external agent. Hobbes justified the necessity of Leviathan on the frailty of mere words. Hobbes calls a contract that involves a promise by at least one of the parties to perform in the future a *covenant* (1960, 87). When both parties promise future performance, it is a *covenant of mutual trust* (p. 89). A covenant of mutual trust in a state of nature is void in Hobbes's view if either has a reasonable suspicion that the other will not perform: "For he that performeth first, has no assurance the other will perform after; because the bonds of words are too weak to bridle men's ambition, avarice, anger, and other passions, without the fear of some coercive power; which in the condition of mere nature, where all men are equal, and judges of the justness of their own fears, cannot possibly be supposed" (pp. 89–90). On the other hand, a covenant made "where there is a power set up to constrain those that would otherwise violate their faith" is likely to be fulfilled (p. 90). Thus, Hobbes argued for the necessity of a "coercive power, to compel men equally to the performance of their covenants, by the terror of some punishment, greater than the benefit they expect by the breach of their covenant" (p. 94).

The weakness of mere words and the necessity of external agents to enforce contracts is also a foundation upon which the powerful edifice of noncooperative game theory has been constructed. John Nash (1950, 1951) distinguished between cooperative and noncooperative games. In cooperative games, players can communicate freely and make enforceable agreements; in noncooperative games, they can do neither. Some theorists particularly stress the inability to make enforceable agreements: "The decisive question is whether the players can make enforceable agreements, and it makes little difference whether they are allowed to talk to each other. Even if they are free to talk and to negotiate an agreement, this fact will be of no real help if the agreement has little chance of being kept. An ability to negotiate agreements is useful only if the rules of the game make such agreement binding and enforceable" (Harsanyi and Selten 1988, 3).[1] In other words, communication without a change in the payoff function does not eliminate a Nash equilibrium (the major solution concept used in noncooperative game theory). Not all Nash equilibria are efficient. Equilibrium payoffs are lower in a finitely repeated, full-information dilemma game than if the participants could make credible ex ante commitments. Hobbes's state of nature has frequently been represented as a social dilemma game.[2]

The assumption that what is called a *sword* wielded by an "external enforcer" is necessary before individuals can make credible ex ante commitments in social dilemma situations has important implications for common pool resources. Common pool resources (CPRs) are natural or man-made resources whose yield is subtractable and the exclusion from which is nontrivial (but not necessarily impossible).[3] Because CPRs can be represented as social dilemmas, particular policy recommendations follow almost immediately. Ophuls argued, for example, that "environmental problems cannot be solved through cooperation . . . and the rationale for government with major coercive powers is overwhelming" (1973, 228). His conclusion was that "even if we avoid the tragedy of the commons, it will *only* be by recourse to the tragic necessity of Leviathan" (p. 220).

Empirical evidence suggests, however, that individuals facing social dilemmas in many cases develop credible ex ante commitments without relying on external authorities. *Appropriators* from CPRs (e.g., fishers, irrigators, and herders) have repeatedly shown their capacity to organize themselves, establish credible commitments, monitor each others' behavior, and impose sanctions on those who break their commitments.[4] Self-organized CPR institutions have been devised without reference to central authorities and sustained over long periods of time without enforcement by external agents.[5] Experimental studies have repeatedly found that individuals placed in laboratory social dilemmas and allowed to communicate consistently achieve better outcomes than predicted by noncooperative game equilibrium. While findings concerning the positive effect of communication in laboratory settings (covenanting without a sword) lead to optimistic predictions about the capacities of individuals to solve social dilemmas, many cases of self-organized arrangements in field settings rely on internal sanctioning mechanisms (E. Ostrom 1990).

Past research has produced three anomalies from the standpoint of predictions derived from both Hobbes's state-of-nature theory and noncooperative game theory:

1. In one-shot social dilemma experiments, communication alone leads to substantial improvement in outcomes.[6]
2. In repeated social dilemma experiments, repeated communication alone leads to substantial improvements in joint outcomes.[7]
3. In field settings of repeated social dilemmas, participants invest substantial time and effort monitoring and imposing sanctions on one another.[8]

The results from field settings show that participants in social dilemmas do not rely entirely on communication. They monitor each other closely and impose sanctions on those who do not conform to the rules they have devised. If communication alone were a fully reliable mechanism to overcome the gap between ex post temptations and ex ante promises, then one should not see time and effort devoted to monitoring and sanctioning efforts in the field. Thus, prior research has generated interesting puzzles. Given that social dilemmas lie at the foundation of the theory of the state and the theory of collective action, it is important to explore the independent and interactive effects of (1) communicating (covenanting) alone, both one-shot and repeated; (2) sanctioning (the sword) alone; and (3) communicating with options to sanction (covenants with a sword). We shall manipulate experimental treatments to examine all three mechanisms. We shall construct a common constituent game as the basis for all these manipulations. This game, an n-person CPR appropriation game, will now be described and solved.

GAME-THEORETICAL PREDICTIONS

The Common Pool Resource Constituent Game

We will first specify the class of constituent CPR games from which we draw our designs. Assume a fixed number n of appropriators with access to the CPR. Each appropriator i has an endowment of resources e which can be invested either in the CPR or a safe, outside activity. The marginal payoff of the outside activity is normalized equal to w. The payoff to an individual appropriator from investing in the CPR depends on aggregate group investment in the CPR and on the appropriator investment as a percentage of the aggregate. Let x_i denote appropriator i's investment in the CPR, where $0 \leq x_i \leq e$. The group return to investment in the CPR is given by the production function $F(\Sigma x_i)$, where F is a concave function, with $F(0) = 0$, $F'(0) > w$, and $F'(ne) < 0$. Initially, investment in the CPR pays better than the opportunity cost of the forgone safe investment [$F'(0) > w$]; but if the appropriators invest all resources in the CPR the outcome is counterproductive [$F'(ne) < 0$]. Thus, the yield from the CPR reaches a *maximum net level* when individuals invest some, but not all, of their endowments in the CPR.[9] This environment is much richer and more complex than the ubiquitous repeated prisoner's dilemma game. While no formal game or laboratory experiment ever captures all the nuances of field settings, this n-person CPR game is a far more realistic environment in which to investigate the questions we have posed than many of the dilemma games previously explored.

Let $x = (x_1, \ldots, x_n)$ be a vector of individual appropriators' investments in the CPR. The payoff to an appropriator, $u_i(x)$, is given by

$$\begin{aligned} u_i(x) &= we \text{ if } x_i = 0 \\ &= w(e - x_i) + (x_i/\Sigma x_i)F(\Sigma x_i) \text{ if } x_i > 0\,. \qquad (1) \end{aligned}$$

Equation 1 reflects the fact that if appropriators invest all their endowments in the outside alternative, they get a sure payoff (we), whereas if they invest some of their endowments in the CPR, they get a sure payoff $w(e - x_i)$ plus a payoff from the CPR, which depends on the total investment in that resource $F(\Sigma x_i)$ multiplied by their share in the group investment $(x_i/\Sigma x_i)$.[10] Let the payoffs (represented in equation 1) be the payoff functions in a symmetric, noncooperative game. Since our experimental design is symmetric, there is a symmetric Nash equilibrium, with each player investing x_i^* in the CPR, where

$$-w + (1/n)F'(nx_i^*) + F(nx_i^*)[(n - 1)/x_i^*n^2] = 0\,. \qquad (2)$$

At the symmetric Nash equilibrium, group investment in the CPR is greater than optimal, but not all yield from the CPR is wasted.

Compare this deficient equilibrium to the optimal solution. Summing across individual payoffs $u_i(x)$ for all appropriators i, one has the group payoff function

$$u(x) = nwe - w\Sigma x_i + F(\Sigma x_i)\,,$$

which is to be maximized subject to the constraints $0 \leq \Sigma x_i \leq ne$. Given the specified productivity conditions on F, the group maximization problem has a unique solution characterized by the condition:

$$-w + F'(\Sigma x_i) = 0. \qquad (3)$$

According to equation 3, the marginal return from a CPR should equal the opportunity cost of the outside alternative for the last unit invested in the CPR.

It is worth noting that both the Nash equilibrium investment and the optimum group investment do not depend on the endowment parameter e as long as e is sufficiently large. Out of equilibrium, however, a larger e means players are capable of making larger mistakes. If the time required to converge to an equilibrium is large, then the Nash equilibrium may be an inappropriate behavioral theory for CPRs with high endowments.

Finite Repetition of a Common Pool Resource Constituent Game

Denote the constituent game by X and let X be played a finite number of times. Typically, a repeated game has many equilibria to choose from. Two equilibrium refinement principles are subgame perfection and subgame consistency. An equilibrium is subgame perfect if it prescribes equilibrium play on every subgame. An equilibrium is subgame consistent if it prescribes identical play on identical subgames. If the constituent game has a unique equilibrium, then the finitely repeated game has a unique subgame perfect and subgame consistent equilibrium (Selten 1971). Thus, equation 2 characterizes a finite sequence of equilibrium outcomes. This prediction is based on the assumption of a finite game of complete information. Our subjects know the game is finite.[11] Although we do not have complete control over our subjects' understanding of their decision task, we make all information readily available to them. Failure to induce complete information on the part of our subjects jeopardizes the uniqueness of this refined equilibrium (Kreps et al. 1982).

Communication and the Constituent Game

When the constituent game X has a unique equilibrium x^*, neither repetition nor communication creates new equilibrium outcomes. Let c denote a communication strategy, in the communication phase C, available to any player. As long as saying one thing and doing another has no payoff consequences, then any strategy of the form (c, x^*) is an equilibrium of the one-shot game (C, X), and finitely repeated x^* is a subgame consistent equilibrium outcome of one-shot communication (C, X, X, . . . , X) or repeated communication (C, X, C, X, . . . , C, X). In this situation, subgame consistency is deaf to covenants. However, as we shall show, communication makes a big difference in behavior.

Sanctioning and the Constituent Game

Our sanctioning institution is represented formally using the following construction. Let s be a matrix of zeros and ones, where $s_{ij} = 1$ means that player i has sanctioned player j, and $s_{ij} = 0$ means that i has not sanctioned j. Row i of the matrix s codes all of player i's sanctioning behavior. As before, let x be a vector of individual investments in the CPR and $u_i(x)$ be i's payoff function in the game without sanctioning. Player i's payoff function in the game with sanctioning, $u_i(x, s)$, is given by

$$u_i(x, s) = u_i(x) - f1 \sum_j s_{ij} - f2 \sum_j s_{ji}. \qquad (4)$$

The parameters $f1$ and $f2$ represent the cost of fining and being fined, respectively.[12] The sum Σs_{ij} is the total number of fines j levied by player i, costing him $f1$ each; the sum Σs_{ji} is the total number of times player i is fined, costing him $f2$ each.

Adding this sanctioning mechanism to our constituent game X produces a game X–S with a unique subgame consistent equilibrium. In a one-shot game with a unique Nash equilibrium x^*, any sanctioning activity is costly and cannot lead to higher payoffs. Thus, the equilibrium of the one-shot game with sanctioning is the pair $(x^*, S^*) = (x^*, 0)$; that is, the equilibrium sanctioning matrix is the zero matrix. At equilibrium, no one sanctions. Now suppose that the one-shot CPR game with sanctioning is to be repeated a finite number of times T. This finitely repeated game has a unique subgame consistent equilibrium given by strategy 1:

STRATEGY 1. *In every round, play (x^*, 0). In the event of any deviation from prescribed play, resume playing (x^*, 0) after the deviation.*

This equilibrium follows from backward induction. At the last round, T, no deviation is profitable. At the next to last round, $T - 1$, given that no deviations will occur in the last round, then no deviation is profitable—and so on. Repeating the game should not lead to sanctioning, either. There is, however, compelling evidence that backward induction must be learned through repeated play.

Besides the unique subgame consistent equilibrium, there are many imperfect equilibria as well. Let $z_i < x_i^*$ be the same for all i. Consider the repeated game strategy, strategy 2:

STRATEGY 2. *In every round except T, play (z, 0). In the event of any deviation, play ($x_i = e$, $s = I$) for one round, then resume playing (z, 0). If no deviation took place in round $T - 1$, play (x^*, 0) in round T.*

This represents a trigger strategy. All players agree to invest less than they would according to strategy 1. If some player cheats, then every player dumps all his resources into the CPR ($x_i = e$) and every player issues one sanction for one round. Then play returns to normal. In the final round, everyone plays the one-shot Nash equilibrium. We claim that strategy 2 represents an imperfect equilibrium. To show this, it suffices to show that no deviation from prescribed play pays. Let $F(ne)$ be a very large negative number. For $f1$ and $f2$ large enough, a player who deviated optimally for one round would gain some positive amount, depending on the level of z_i but in the next round would lose $(1/n)F(ne) + f1 + f2$ due to punishment from overinvestment and sanctions, as in strategy 2. This threat we call the *dire threat*, since it is the worst threat imaginable for one round in our design. Given such a threat, it does not pay to deviate, even for one round. Finally, if a punishment is not called for in the last round, the endgame equilibrium is played in that round. This shows that strategy 2 is an equilibrium. Its imperfection lies in the fact that the trigger punishment—dumping all tokens into the resource, everybody placing a fine—is too harsh to be credible at the end of the game.[13]

There is a large set of equilibria along the lines of strategy 2 involving variation of the length of punishment (1 or more rounds), the base level of investments z_i, and the direness of the one-period threat (dump not quite all tokens in the CPR, levy fines with some probability). In particular, by varying $f1$ and $f2$, we hoped to allow the subjects to find equilibria of the strategy 2 family involving punishments of the form (z_i, I) (i.e., reduced investment in the CPR) but sanctions for everyone if a deviation occurs (see Jankowski 1990).

Communication, the Constituent Game, and Sanctioning

We investigate the combination of communication with sanctioning in two ways. Our first design allows for a one-shot communication period, which is then followed by a sequence of constituent games with a sanctioning mechanism imposed. In our second design, we impose a one-shot communication period in conjunction with an opportunity for the subjects to choose whether or not they want a sanctioning mechanism. In both designs, the payoff functions are still given by equation 4, since communication per se has no payoff consequences and sanctioning does. Without loss of generality, let c be a communication strategy. Then appending c to strategy 1 yields a subgame consistent equilibrium, and every subgame consistent equilibrium has the same payoffs as strategy 1. In addition, as in repeated X–S, imperfect equilibria exist yielding higher payoffs than equilibria which are subgame consistent.

THE LABORATORY DECISION ENVIRONMENT

Design

In our experimental investigation we have operationalized this CPR environment with eight appropriators ($n = 8$) and quadratic production functions $F(\Sigma x_i)$, where

$$F(\Sigma x_i) = a\Sigma x_i - b(\Sigma x_i)^2,$$

with $F'(0) = a > w$ and $F'(nw) = a - 2bnw < 0$. For this quadratic specification, one has from equation 3 that the group optimal investment satisfies $\Sigma x_i = (a - w)/2b$. The CPR yields 0% on net when investment is twice as large as optimal, $\Sigma x_i = (a - w)/b$. Finally, solving equation 2, the symmetric Nash equilibrium group investment is given by

$$\Sigma x_i = [n/(n + 1)](a - w)/b.$$

This level of investment is between maximal net yield and zero net yield, approaching the latter as n gets large. One additional constraint that arises in a laboratory setting is that the x_i be integer-valued. This is accomplished by choosing the parameters a, b, d, and w in such a way that the predictions associated with Σx_i are all integer-valued.

In particular, we use the parameters shown in Table 1. These parameters lead to the predictions that 36 tokens invested in Market 2 yields the level of investment that maximizes group earnings and that each subject investing 8 tokens in Market 2 is a unique Nash equilibrium. At this equilibrium, subjects earn approximately 39% of maximum net yield from the CPR. Once again, note that the Nash equilibrium and optimal investment are not affected by the level of endowments.

Subjects and the Experimental Setting

Our experiments used subjects drawn from the undergraduate population at Indiana University. Students were volunteers recruited from principles-of-economics classes. Prior to recruitment, potential volunteers were given a brief explanation in which they were told only that they would be making decisions in an "economic choice" environment and that the money they earned would be dependent upon their own investment decisions and those of the others in their experimental group. In all experiments reported here, subjects were randomly recruited from a pool of subjects with prior experience in a CPR decision environment. All experiments were conducted on the NOVANET computer system at Indiana University. The computer facilitates the accounting procedures involved in the experiment, enhances across-experiment subject control, and allows for minimal experimenter involvement.

We shall represent the state of nature with the minimal institution baseline game (no covenants/no swords), then present findings from three environments with institutional configurations: (1) an imposed communication mechanism, (2) an imposed sanctioning mechanism, and (3) an imposed communication mechanism with either an imposed sanctioning mechanism or an opportunity to choose a sanctioning mechanism. Figure 1 gives an overview of the experimental design. Table 2 presents a summary of results across designs, focusing on the average net yield from the CPR (Market 2) as a percentage of maximum possible net yield.[14]

TABLE 1

Experimental Design Baseline Parameters for a Given Decision Period

EXPERIMENT CHARACTERISTICS	EXPERIMENT TYPE LOW-ENDOWMENT	EXPERIMENT TYPE HIGH-ENDOWMENT
Number of subjects	8	8
Individual token endowment	10	25
Production function, Market 2[a]	$23(\Sigma x_i) - .25\,(\Sigma x_i)^2$	$23(\Sigma x_i) - .25(\Sigma x_i)^2$
Market 2 return/unit of output	1¢	1¢
Market 1 return/unit of output	5¢	5¢
Earnings/subject at group max.[b]	91¢	83¢
Earnings/subject at Nash equil.	66¢	70¢
Earnings/subject at 0 net yield	50¢	63¢

[a] Σx_i = the total number of tokens invested by the group in Market 2. The production function shows the number of units of output produced in Market 2 for each level of tokens invested therein. Market 2 represents a CPR because the total output from Market 2 (and each subject's share of that output) is a function of the investment levels of all subjects. [b] In the high-endowment design, subjects were paid in cash one-half of their "computer" earnings. This maintained potential experimental profits at near equal levels across designs. Amounts shown are potential cash payoffs.

THE BASELINE GAME: NO COVENANTS AND NO SWORDS

For comparison with the designs discussed later, the baseline experiments can be represented as an iterated series (of 20 rounds) of the constituent game X. At the beginning of each experimental session, subjects were told that (1) they would make a series of investment decisions, (2) all individual investment decisions were anonymous to the group, and (3) they would be paid their individual earnings (privately and in cash) at the end of the experiment. Subjects then proceeded at their own pace through a set of instructions summarized as follows:

> Subjects faced a series of decision rounds in which they were endowed with a specified number of tokens, which they invested between two markets. Market 1 was described as an investment opportunity in which each token yielded a fixed (constant) rate of output and each unit of output yielded a fixed (constant) return. Market 2 (the CPR) was described as a market that yielded a rate of output per token dependent upon the total number of tokens invested by the entire group. The rate of output at each level of group investment was described in functional form, as well as tabular form. Subjects were informed that they would receive a level of output from Market 2 that was equivalent to the percentage of total group tokens they invested. Further, subjects knew that each unit of output from Market 2 yielded a fixed (constant) rate of return.[15]

Subjects knew with certainty the total number of decision makers in the group, total group tokens, and that endowments were identical. Subjects knew that the more the group invested in Market 2, the lower the average and marginal returns from Market 2 would be. They did not know the exact number of decision rounds.

In the baseline experiments, subjects participated in a series of 20 decision rounds. After each round, subjects were shown a display that recorded (1) their profits in each market for that round, (2) total group investment in Market 2, and (3) a tally of their cumulative profits for the experiment. During the experiment, subjects could request, through the com-

FIGURE 1.

Experimental design

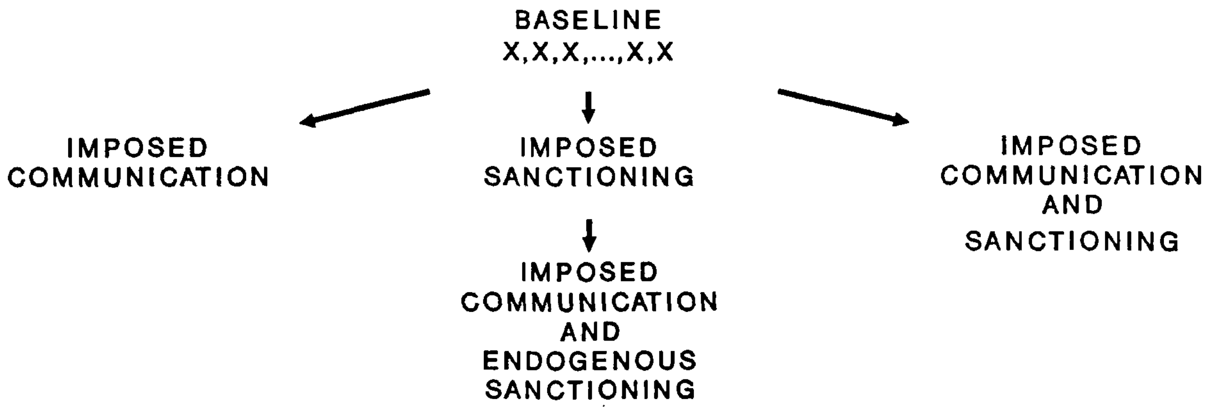
BASELINE
X,X,X,...,X,X
IMPOSED COMMUNICATION
IMPOSED SANCTIONING
IMPOSED COMMUNICATION AND SANCTIONING
IMPOSED COMMUNICATION AND ENDOGENOUS SANCTIONING

TABLE 2

Summary Results: Average Yield as a Percentage of Maximum

	ROUND					
EXPERIMENTAL DESIGN	1–5	6–10	11–15	16–20	21–25	26+
Baseline						
10TK[a]	51.5	34.7	34.4	35.6	37.1	29.6
25TK	−42.5	−12.4	10.3	32.0	—	—
One-shot communication 25TK	−40.9	−12.7	74.1	45.4	42.5	58.6
Repeated communication						
10TK	32.6	27.3	97.2	98.4	100.0	—
25TK	32.5	−14.4	74.1	75.0	68.9	—
Sanction 25TK	−35.7	−39.6	40.1	38.8	28.7	—
One-shot communication sanction 25TK	−.7	−27.0	86.8	86.3	82.5	77.8
One-shot communication 25TK						
No sanction chosen[b]	46.4	41.2	91.7	61.9	14.7	—
Sanction chosen[b]	−16.9	−5.1	92.5	91.6	89.9	93.8
Sanction chosen[c]	—	—	96.8	97.0	96.7	90.4

[a] TK corresponds to tokens per subject.

[b] Communication and sanctioning choice occurred after round 10.

[c] Communication and sanctioning choice occurred after round 1; the table displays this data beginning in round 11 for comparison purposes.

puter, this information for all previous rounds. Subjects received no information regarding other subjects' *individual* investment decisions or the number of rounds.

As shown in Table 2, in the first 10 rounds, the low-endowment treatment leads to an average net yield of 43%. In the high-endowment treatment, net yield averages −28%. This shows the importance of the endowment treatment out of equilibrium. In the last five rounds of the baseline experiments, the two treatments are very close—36% and 32%, respectively. These percentages are fairly close to the 38% predicted by the subgame consistent equilibrium, evidencing strategic learning on the part of the subjects.

Several characteristics of the individual experiments are important. Investments in Market 2 are characterized by a pulsing pattern: yield is reduced, investors reduce their investments in Market 2, and yields increase. For the high-endowment experiments, the low points in the pulsing pattern were at yields far below zero. There was some tendency for the variance in yields to decrease over the course of the experiments. At the aggregate level, the Nash prediction best describes our data. However, at the individual level we observed no experiment in which individual investments stabilized at the Nash equilibrium. In short, as predicted by both Hobbes's state-of-nature theory and subgame consistent equilibrium, individuals acting independently in a CPR game without communication or sanctioning do not solve the collective action problem they confront.

COVENANTS WITHOUT A SWORD: COMMUNICATION ALONE

Prior experimental evidence strongly supports the conclusion that communication in social dilemmas increases the frequency with which players choose and sustain joint income-maximizing strategies even when individual incentives conflict with such strategies. We shall examine the results from several types of communication experiments.

One-Shot Communication

Our first communication design was parallel to that of the high-endowment baseline game for the first 10 repetitions of the constituent game.[16] At the end of the tenth round, the subjects were informed that they would have a single opportunity of 10 minutes to discuss the decision problem. The instructions were:

> Some participants in experiments like this have found it useful to have the opportunity to discuss the decision problem you face. You will be given ten minutes to hold such a discussion. You may discuss anything you wish during your ten-minute discussion period, with the following restrictions: (1) you are not allowed to discuss side payments; (2) you are not allowed to make physical threats; (3) you are not allowed to see the private information on anyone's monitor.

After this opportunity to communicate, the subjects returned to the constituent game, which was then repeated up to 22 more times. The structure of the one-shot communication experiment is

$$X, X, X, \ldots, X, C, X, X, \ldots, X.$$

The subgame consistent and subgame perfect equilibrium outcome for the one-shot communication game is for each individual to invest 8 tokens in the CPR, the same as in the baseline. The maximum net yield is obtained if a total of 36 tokens are invested. Subjects are not allowed to invest fractional tokens; and the symmetric strategy to obtain the maximum return is halfway between everyone's investing 4 tokens and everyone's investing 5 tokens. If the subjects were to decide to invest either 4 or 5 tokens each, they would obtain 99% of maximum net yield in either case.

The transcripts of the discussion during the communication round reveal that subjects perceived their problem as involving two tasks: determining the maximal yield available and agreeing upon a strategy to achieve that yield. In all three groups, an agreement was reached; but no group found the optimal solution. Averaging over all experiments, yields were negative throughout the first 10 rounds (−27%). Once communication was allowed, net yields jumped to an average of 74% for the five rounds immediately following the communication time-out, then gradually decayed thereafter. The average net yield in the postcommunication phase was 55%. Defining a *defection* as an individual Market 2 investment greater than that agreed upon, we observe a defection rate of 25% during the decision rounds following communication. These results suggest that a single communication period enables participants to begin the process of adopting a joint strategy. However, the inability to communicate more often limits the durability of their agreements.

Repeated Communication

Our second design involves repeated communication in both the low- and high-endowment settings. At the outset, the constituent game was repeated for 10 rounds. After round 10, the subjects read an announcement similar to that for one-shot communication but informing them they would have an opportunity for discussion after *each* subsequent round. The subjects left their terminals and sat facing one another.[17] The structure of the repeated communication experiment is

X, X, . . . , X, C–X, C–X, . . . , C–X.

These experiments provide strong evidence for the power of repeated face-to-face communication. Subjects successfully used the opportunity to (1) calculate coordinated yield-improving strategies, (2) devise verbal agreements to implement these strategies, and (3) deal with nonconforming subjects through verbal statements. When allowed to communicate repeatedly, subjects greatly enhanced their joint yield and sustained this enhancement. In the *low*-endowment environment, net yields averaged 99% of optimum in the repeated communication phase with a 5% defection rate.[18] The *high*-endowment CPR game is a more challenging decision environment than the low-endowment one. While the subgame consistent equilibrium outcomes of the two games are identical, the disequilibrium implications of the 25-token (high-endowment) game change considerably. With 25 tokens, as few as three subjects investing all of their tokens can essentially ruin the CPR (bring returns below w), while with 10 tokens, it takes seven out of eight subjects to accomplish this much damage. Net yields in the repeated communication phase averaged only 73% of optimum, with a 13% defection rate.

Repeated communication enabled subjects to discuss defections and to cut the defection rate on agreements significantly. In all communication experiments, subjects offered and extracted promises of cooperation, thereby increasing their joint yield significantly above that obtained prior to communication. Discussions went well beyond discovering the level of investments that would generate maximum yields. A striking aspect of the discussion rounds was how rapidly the subjects, who had not had an opportunity to establish a well-defined community with strong internal norms, were able to devise their own agreements and verbal punishments for those who broke those agreements. These verbal sanctions had to be directed at unknown defectors, since subjects' decisions were anonymous. Subjects detected defection solely through aggregate investments. In many cases, statements like "Some scum-bucket is investing more than we agreed upon" were a sufficient reproach to change defectors' behavior. However, verbal sanctions were less effective in the 25-token environment.

The evidence from the one-shot and the high-endowment designs suggests why individuals in many field settings may not rely only on face-to-face communication. When repeated discourse is infeasible or when the actions of one or a few individuals can be a strong disequilibrating force, individuals who have the capacity to agree to sanction one another, as well as communicate with one another, might well want to reinforce their covenants with their own swords. While subgame consistency predicts that individuals in such settings would not sanction one another, endogenous sanctioning is frequently observed in field settings. We now turn to an examination of whether sanctioning behavior will occur in our laboratory environment and its effects on behavior.

SWORD WITHOUT COVENANTS: SANCTIONING ALONE

Experiments in this design began like high-endowment baseline experiments with the exception that after each round, subjects received *individual* data of all decisions.[19] This information was given by subject number, thus maintaining anonymity. Our sanctioning mechanism required that each subject incur a cost (a fee) in order to sanction another. In our first sanctioning design, after round 10, subjects were given an announcement summarized as follows:

> Subjects were informed that in all remaining rounds each would be given the opportunity to place a fine. Each subject could levy one fine at a specified fee. The subject fined would pay a fine of a specified amount. It was possible for a single subject to be charged multiple fines. After each round, each subject filled out a fining form. These forms were collected and tallied by the experimenter, who then reported the results privately to each subject. Note that any subject who was fined did not know the identity of those who imposed the fine. At the end of the experiment, the experimenters subtracted from subjects' total profits the total of all fees and all fines.

The actual fees ranged from 5¢ to 40¢, the fines from 10¢ to 80¢ cents. The fee-to-fine ratio was either .25 or .50. After subjects read the announcement, we answered questions regarding the implementation of the procedure. No discussion was held on why the subjects might want to use the procedure or its possible consequences. This created an experimental setting as close as possible to the noncooperative assumptions of no communication and no capacity to engage in enforceable agreements. The structure of the imposed sanctioning design is

X, X, . . . , X–S, X–S, X–S, . . . , X–S, X.

The principle results from our sanctioning experiments are summarized as follows.[20] Across all eight experiments, net yield rose from −38% before the imposition of sanctioning to 37% after. When one subtracts the costs of fees and fines, however, average net yield increased to only 9%. Thus, sanctioning alone is not an efficient institution. Besides these quantitative results, we draw the following qualitative conclusions:

1. Significantly more sanctioning occurs than predicted by subgame consistency, and the frequency is inversely related to cost.
2. Sanctioning is primarily focused on heavy CPR investors.
3. There is a nontrivial amount of sanctioning that can be classified as error, lagged punishment, or "blind" revenge.

We observed 176 instances of sanctioning across the eight experiments. In no experiment did we observe fewer than 10 instances. The frequency of sanctions was inversely related to the cost of imposing the fine and dramatically increased with the stiffness of the fine. Further, our results, although reminiscent of strategy 2, did not strictly support the conclusion that subjects were playing an equilibrium of this form. Except for one experiment, where net yield was over 95%, yields were too low, and sanctioning levels too high, to be consistent with imperfect equilibria.

The second and third results relate to the reasons for sanctioning. From postexperiment interviews and personal observations, we offer four explanations for the higher-than-predicted level of sanctioning:

1. *One-round punishment.* The person fined was the highest, or one of the highest, investors in the previous round.
2. *Lagged punishment.* The person fined was one of the highest investors in the CPR in a round prior to the previous one.
3. *Blind revenge.* The person fined was a low CPR investor and was fined by a person fined in a previous round.
4. *Error.* No obvious explanation can be given for the action (trembling hand).

In summary, 77% of all sanctioning was aimed at investors who in the previous round were above-average investors in the CPR. An additional 7% was aimed at subjects who had been heavy investors in the CPR in earlier (but not the most recent) rounds. We classify 5% as blind revenge and the remaining 11% as errors.[21]

The evidence from these experiments suggests why individuals in field settings might not want to rely on sanctioning alone. Individuals who have the capacity to sanction one another without the ability to communicate about joint strategies and the use of sanctions face an insuperable handicap to increasing efficiency. Finally, we return to the question whether communication and sanctioning together foster sustainable high yields.

COVENANTS WITH A SWORD: COMMUNICATION AND SANCTIONING

Our last two decision environments investigate the consequences of combining a one-shot opportunity to communicate with either an experimenter-imposed sanctioning mechanism or an opportunity to decide whether or not to adopt a sanctioning mechanism endogenously. These experiments began like those in the design with sanctioning alone. After round 10, subjects were given an announcement that they would have a single 10-minute discussion period. In experiments with an imposed-sanctioning mechanism, subjects were also given an announcement (prior to discussion) similar to that of the sanctioning-alone environment. In experiments where subjects had an opportunity to choose a sanctioning mechanism, the announcement informed them that at the end of 10 minutes they would vote on whether to institute a sanctioning mechanism and if so, what the level of fines would be. The only restriction on the sanctioning mechanism was that the fee-to-fine ratio was ½. The voting rule was strict majority, with the status quo a repeated baseline experiment without a sanctioning mechanism.

Imposed Communication and Sanctioning

The structure of the three experiments in this design is

X, X, . . . , X, C, X–S, X–S, . . . X–S, X.

In the first experiment, the participants rapidly focused on the problem of deciding upon a joint investment strategy. They spent most of their 10 minutes calculating various options to ensure that they had discovered an optimal strategy. They decided to invest 4 tokens each in Market 2 and the remaining 21 tokens in Market 1. Further, they agreed to fine one another if anyone put more than 4 tokens in Market 2. One subject said, "If everyone puts in 4 tokens, we are going to be making 42 cents more money in the individual accounts. This is the highest." Another said, "Does everyone agree to this? OK, now we have agreed that everyone will put 4 tokens in and if anyone puts any more in, we are all going to fine them. Is that all agreed, now?" With this specific agreement to which everyone nodded assent, the subjects returned to their terminals and made investments for 16 more rounds without a single defection nor any use of the sanctioning mechanism. They obtained 98% of maximum net yield from the CPR and did not waste resources on fees or fines.

In the second experiment, the subjects did not find the optimal strategy but devised a complex rotation system to ensure that they all received what they thought would be maximal returns. They decided that each subject would invest 6 tokens in Market 2 and that the right to invest 2 more tokens each round would be rotated first to Subject 1, then to Subject 2, on through Subject 8. One subject suggested that they not fine at all, but another argued: "No, let's fine anyone who breaks our rules. If they break our rules, then we should fine 'em!" After further discussion, the subjects agreed that they would use the sanctioning mechanism to fine anyone who deviated from "their rules." For two rounds they kept their agreement. On round 13, one subject invested 7, rather than 6, and was immediately fined by one of the other subjects. On round 19, two subjects invested one more token than agreed upon and each was immediately sanctioned by another subject. No more defections were attempted and no more fees were paid to assess fines. In this experiment, the subjects achieved 86% of maximum net yield (since they had miscalculated the optimum). Their net return dropped to 79% when fees and fines were deducted from their earnings.

In the third experiment, the subjects never discussed the possibility of devising a joint strategy even though they mentioned how the overinvestment of some of the subjects during the first 10 rounds had made it difficult for the rest of them. The closest they got to an agreement was to discuss fining those who were obviously overinvesting, for example, "those who invested over 21 tokens in Market 2." A considerable amount of their discussion time was wasted in awkward silence. They finally asked whether they had to sit there the entire 10 minutes. After verifying that no subject wanted to use the remaining 2 minutes of their time for further discussion, the experimenters let the subjects return to their terminals. Following the communication period, the subjects achieved an average of 70% of net yield from the CPR, up from −14% in prediscussion rounds. A total of twenty 40¢ fees were paid to impose the same number of 80¢ fines on other subjects. The fines were directed toward subjects who had invested heavily in Market 2 in the prior round. Net yield fell to 24% with fees and fines deducted from earnings.

Imposed Communication and Endogenous Sanctioning

In order to make the choice of a sanctioning mechanism meaningful, subjects in this decision environment were randomly drawn from the pool of subjects from our imposed sanctioning design. The structure of the four experiments in this design is

X, X, . . . , X, C, X, X, . . . , X or

X, X, . . . , X, C, X–S, X–S , . . . , X–S, X.

Subjects decided upon a joint investment strategy and established a sanctioning mechanism in only two of the four experiments. In one they chose a fee-to-fine ratio of 10¢/20¢ and in the other a ratio of 20¢/40¢. Net yield averaged 91% of maximum following the communication round. The level of defection from their agreements was very low. Net yield fell slightly to 86% when fees and fines were deducted.

In one of the experiments where subjects rejected the adoption of a sanctioning mechanism, they did adopt an agreed-upon joint strategy to invest four tokens in the CPR (a joint strategy very close to optimal). In the first few rounds following discussion, they sustained their agreement; but by round 16, four subjects invested more than their agreement. From then on, the level of defections steadily rose. Overall, they had an average net yield of 36% and a defection rate of 42%, representing the highest defection rate of any communication experiment where an agreement was reached. The other experiment in which a sanctioning mechanism was not chosen is an outlier. Across all of our CPR experiments with the baseline institution, this is the only experiment where yields in the first 10 rounds were essentially maximal. When given the opportunity to discuss the decision problem and choose a sanctioning mechanism, the group agreed that they did not need a mechanism and that no one should try to get "greedy," that is, invest too much in the CPR. The group held together for a few rounds, after which yields began a gradual decline. This decline was due primarily to a gradual increase in Market 2 investments by two subjects. By round 25, net yield had dropped to 56% of optimum. Net yield for all rounds following the discussion session averaged 76%, compared to 90% prior to round 10.

We have traced back to the specific sanctioning/no communication experiment in which each of these subjects participated. Of the 32 subjects in these four experiments, 18 voted for, and 14 voted against, the implementation of a sanctioning mechanism. Of the 14 who voted *no,* 11 had previously participated in a sanctioning experiment with a fee-to-fine ratio of 20¢/80¢. Of the 18 who voted *yes,* only 3 had been in a 20¢/80¢ design. We infer from this result that the high level of sanctioning activity in the 20¢/80¢ design, the lack of overall efficiency gains and the presence of blind revenge combined to impede the willingness of participants to choose a sanctioning mechanism.

It is possible that the experience of the first 10 rounds of the constituent game had an effect on mechanism choice. To examine this possible hysteresis effect, two additional experiments were conducted. In these two experiments, the opportunity to communicate and to adopt a sanctioning mechanism was available at the outset. The structure of these two experiments is

C, X, X, . . . , X or C, X–S, X–S, . . . , X–S, X.

In both of these experiments, the subjects quickly agreed to an investment strategy and a sanctioning mechanism to punish defectors. Across the two experiments, net yields averaged 95%—94% with fees and fines included.

The payoff consequences of selecting or not selecting a sanctioning mechanism were very different across the experiments in this design. The groups choosing some form of sanctioning institution earned average yields of 93% in the postdiscussion phase. Indeed, yields this high suggest that this set of institutions, endogenously chosen, approximate the conditions necessary for a cooperative game. The groups not choosing some form of sanctioning institution earned average yields of only 56%, with serious decay.

The results from this set of communication and sanctioning experiments suggest that some subjects can find yield-improving joint strategies, design a sanctioning mechanism, use the sanctioning mechanism, and achieve a high rate of conformance to their joint strategies. On the other hand, prior negative experience with institutions that individuals view as punitive and inefficient is conducive neither to the design of better institutions nor to a willingness to use them.

CONCLUSIONS: SELF-GOVERNANCE IS POSSIBLE

The inconsistency between predicted results and observed behavior in prior research stimulated this research. In one-shot and finitely repeated games, communication alone is not predicted to have an effect on behavior. Earlier experimental studies of social dilemmas have, however, shown that communication alone leads to more efficient outcomes. We confirm these results in complex CPR environments.

With regard to communication alone, we obtain the following results (summarized in Table 3):

1. In the low-endowment CPR environment, average net yield increased from 35% (when no communication was allowed) to 99% (when communication was allowed on a repeated basis).
2. In the high-endowment CPR environment, average net yield increased from 21% (when no communication was allowed) to 55% (when communication was allowed only once) to 73% (when communication was allowed on a repeated basis).

These results raise several puzzles. Because the subgame consistent equilibrium prediction for games with communication (one-shot or repeated) is the same as that for games without communication, opportunities for "mere jawboning" should make no difference; yet they do. Further, the equilibrium prediction is the same for both low- and high-endowment environments. The high-endowment environment, however, exhibits lower net yield and fosters less effective communication. The finding that CPR appropriators in the field invest substantial resources in sanctioning activities stimulated our exploration of sanctioning behavior in the laboratory. With an imposed sanctioning institution and no communication we find the following:

3. Subjects are willing to pay a fee to place a fine on another subject far more than was predicted.
4. In the high-endowment environment, average net yield increases from 21% with no sanctioning to 37% with sanctioning. When the costs of fees and fines are subtracted from average net yield, however, net yield drops to 9%.

Thus, subjects overuse the sanctioning mechanism, and sanctioning without communication reduces net yield. The finding that CPR appropriators in the field invest substantial resources in both communication and sanctioning activities stimulated our exploration of these joint behaviors in the laboratory. Examining only the high-endowment environment, we find the following:

5. With an imposed sanctioning mechanism and a single opportunity to communicate, subjects achieve an average net yield of 85%. When the costs of fees and fines are subtracted, average net yield is still 67%. These represent substantial gains over the baseline, where the net yield averaged 21%.
6. With the right to choose a sanctioning mechanism and a single opportunity to communicate, subjects who adopt a sanctioning mechanism achieve an average net yield of 93%. When the costs of fees and fines are subtracted, average net yield is still 90%. In addition, the defection rate from agreements is only 4%.
7. With the right to choose a sanctioning mechanism and a single opportunity to communicate, subjects who do not adopt a sanctioning mechanism achieve an average net yield of only 56%. In addition, the defection rate from agreements is 42%.

Thus, subjects who use the opportunity to communicate to agree to a joint investment strategy and

TABLE 3

Aggregate Results All Designs (%)

EXPERIMENTAL DESIGN	AVERAGE NET YIELD CPR	AVERAGE NET YIELD CPR (MINUS FEES AND FINES)	DEFECTION RATE
Baseline			
10TK[a]	34	—	—
25TK	21	—	—
One-shot communication 25TK	55	—	25
Repeated communication			
10TK	99	—	5
25TK	73	—	13
Sanction 25TK	37	9	—
One-shot communication sanction 25TK	85	67	1
One-shot communication 25TK			
No sanction chosen	56	—	42
Sanction chosen	93	90	4

Note: All computations are for periods in which the treatment was in effect. Nash equilibrium for all designs is 39% net CPR yield.
[a]TK corresponds to tokens per subject.

choose their own sanctioning mechanism achieve close-to-optimal results. This is especially impressive in the high-endowment environment, where defection by a few subjects is very disruptive. For those who predict cooperation in repeated settings based on trigger strategies, our findings are not supportive. In no experiment where one or more subjects deviated from an agreed-upon joint strategy did the subjects then follow a trigger strategy of substantially increasing their investments in the CPR.[22] In fact, in some experiments where one or more subjects deviated from an agreed-upon joint strategy, some subjects subsequently *reduced* their investments in the CPR. When subjects discussed the problem of how to respond to one or more free-riders, they overtly rejected the idea of dumping all of their tokens into the CPR.[23] To return to our starting point, namely, Hobbes's assertion, these experiments suggest that covenants, even without a sword, have some force, while swords without a covenant may be worse than the state of nature. Best of all the conditions we examined are covenants with an *internal* sword, freely chosen or made available as an institutional option.[24]

Two major implications follow from the results. The first relates to policy analysis. Policymakers responsible for the governance and management of small-scale, CPRs should *not* presume that the individuals involved are caught in an inexorable tragedy from which there is no escape. Individuals may be able to arrive at joint strategies to manage these resources more efficiently. To accomplish this task, they must have sufficient information to pose and solve the allocation problems they face. They must also have an arena where they can discuss joint strategies and perhaps implement monitoring and sanctioning. In other words, when individuals are given an opportunity to restructure their own situation, they frequently—but not always—use this opportunity to make credible commitments and achieve higher joint outcomes without an external enforcer. We cannot replace the determinate prediction of *no cooperation* with a determinate prediction of *always cooperate*. Our findings challenge the Hobbesian conclusion that the constitution of order is only possible by creating sovereigns who then must govern by being above subjects, by monitoring them, and by imposing sanctions on all who would otherwise not comply.[25]

The second major implication relates to behavioral theory. In finitely repeated social dilemma experiments, a wide variety of treatments that do not change the theoretically predicted subgame consistent equilibrium outcomes do change subjects' behavior. This raises a *substantial* question whether our subjects conceptualize their decision task in the way theorists do. For instance, if subjects believe the game is being repeated according to some exogenous probabilistic mechanism, then there are equilibria supporting more cooperative behavior if the subjective continuation probability is not too low. Or it may be that subjects are acting as boundedly rational players in the sense of Selten, Mitzkewitz, and Uhlich (1988). In this case, the observed improvement in yield could be the result of boundedly rational equilibrium, as Selten, Mitzkewitz, and Uhlich observe in a duopoly context. We intend to explore these intriguing possibilities in future research.

Notes

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1. Harsanyi and Selten add that in real life, "agreements may be enforced externally by courts of law, government agencies, or pressure from public opinion; they may be enforced internally by the fact that the players are simply unwilling to violate agreements on moral grounds and know that this is the case" (1988, 3). To model self-commitment using noncooperative game theory, the ability to break the commitment is removed by trimming the branches that emanate from a self-commitment move to remove any alternative contrary to that which has been committed. In a lab setting, this would mean changing the structure of the alternatives made available to subjects after an agreement, which was not done.

2. See, e.g., Campbell 1985; Hardin 1971, 1982; McLean 1981; Moore 1987; Taylor 1987. The term *social dilemma* is introduced by Dawes 1975, 1980.

3. See Bianco and Bates 1990 for a theoretical analysis of the capabilities and limits of assigning leaders strong sanctioning powers and Samuelson et al. 1986 for an experimental investigation of the choice of a Leviathan-like mechanism to solve social dilemmas.

4. See Gardner, Ostrom, and Walker 1990 and E. Ostrom, Gardner, and Walker n.d. for a detailed exposition of the terms we use and the models we have developed.

5. See Berkes 1989; National Research Council 1986; E. Ostrom 1990; Pinkerton 1989; and Wade 1988 for a discussion of successful, failed, and fragile efforts to self-organize and govern, small-scale CPRs. Michael Hechter's (1987) recent synthesis of self-organizing capabilities within many social groups provides evidence across other domains (see also Williamson 1975, 1985). See Kreps 1990 for a useful synthesis of literature relevant to endogenous commitments within firms.

6. The extensive literature on one-shot communication in dilemma games includes Bornstein and Rapoport 1988; Bornstein et al. 1989; Braver and Wilson 1984, 1986; Caldwell 1976; Dawes, McTavish, and Shaklee 1977; Dawes, Orbell, and van de Kragt 1984; Dawes, van de Kragt, and Orbell 1988; Edney and Harper 1978; Jerdee and Rosen 1974; Kramer and Brewer 1986; Orbell, Dawes, and van de Kragt 1990; Orbell, van de Kragt, and Dawes 1988, 1991; van de Kragt et al. 1983, 1986.

7. For a discussion of relevant literature, see Isaac and Walker 1988, 1991; E. Ostrom and Walker 1991.

8. See n. 5.

9. Investment in the CPR beyond the maximum net level is termed *rent dissipation* in the literature of resource economics. One can interpret this environment as a limited access CPR (see, e.g., Clark 1980; Cornes and Sandler 1986; Negri 1989).

10. This specification actually has a number of other possible interpretations. For instance, if one defines $F(\Sigma x_i)/\Sigma x_i = y$ and y to be a public good, then one has the payoff functions for a voluntary contribution mechanism as in Isaac and Walker 1988. Alternatively, one can define y in the same expression to be an externality, in which case one has payoff functions for Plott's experiments on externalities in product markets (Plott 1983). For further details, see Ledyard 1991.

11. During recruitment, subjects are told they will participate in a one-to-two hour decision-making experiment. Although the exact endpoint is not revealed, it is explicitly bounded from above. Further, all subjects have experienced the boundedness of an experiment that lasted between 10 and 30 rounds.

12. We use the word *fine* not in the context of redistribution. What is crucial here is that real resources are used up, not just redistributed, by efforts to sanction.

13. Besides the symmetric imperfect equilibria we have given, there are many asymmetric equilibria. Take any permutation of the identity matrix, e.g., 1 sanctions 2, 2 sanctions 3, etc. Then these permuted sanctions also support the same outcome as strategy 2 does. Notice that if mistakes are what cause deviations, then an equilibrium like strategy 2 will generate n fines every time a mistake takes place—considerably more than the zero fines generated by the subgame consistent equilibrium strategy 1.

14. Net yield accrued as a percentage of maximum = (return from Market 2 − opportunity costs of tokens invested therein)/(return from Market 2 at the optimum − opportunity costs of tokens invested therein). Opportunity costs equal the potential return that could have been earned by investing the tokens in Market 1.

15. A complete set of instructions is available upon request. In high-endowment experiments, subjects were informed that their cash payoff would be one-half of the "lab dollars" earned in the experiment. This was done so that total payments in high- and low-endowment treatments would be approximately equal. At the end of all experiments, subjects were paid privately (in cash) their individual earnings. All subjects had participated previously in an experiment using the constituent game environment. The number of rounds in earlier experiments had varied from 10 to 20. Subjects were recruited randomly from this pool of experienced subjects to ensure that no prior experimental group was brought back intact. We provide a detailed account of behavior in the constituent game environment in Walker, Gardner, and Ostrom 1990, 1991.

16. In the one-shot communication experiments, subjects received information on individual decisions after each round to facilitate our comparing results in designs with sanctioning either alone or in conjunction with one-shot communication added. Information was given by subject number, thereby preserving anonymity. This added information had no significant impact on observed yields.

17. As in the one-shot communication setting, each person was identified with a badge that was unrelated to their player number. This facilitated player identification in our transcripts. If unanimous, players could forgo discussion.

18. We also conducted a series of costly communication experiments, in which subjects had to pay in advance for opportunity to communicate. See E. Ostrom and Walker 1991 for a detailed discussion. See also Isaac and Walker 1988, 1991.

19. Earlier experiments focusing on sanctioning mechanisms without communication include Yamagishi 1986, 1988.

20. A comparison of the initial 10 rounds to the initial 10 rounds of baseline suggests that the addition of anonymous information about individual decisions had no impact on investments.

21. A second set of sanctioning experiments was conducted as a check on the robustness of our original design. Readers of our earlier results conjectured that the lack of a significant improvement in net yield with the introduction of a sanctioning mechanism in our initial design could be due to a hysteresis tied to the decisions in the first 10 periods, periods in which there was no sanctioning mechanism. In our second design, three new experiments were conducted, with the sanctioning mechanism introduced prior to the first decision period. The fee-to-fine ratio was 40¢/80¢. Subjects used fines repeatedly in all three experiments. The results from our second set of experiments are consistent with those from our first design. There was no persistent yield-improving behavior that could be tied to the introduction of the sanctioning mechanism. In fact, when costs of fees and fines are incorporated we found a negative impact on net benefits.

22. It is well known that inducing trigger strategy behavior in subjects is extremely difficult. For a recent attempt, see Sell and Wilson 1991.

23. One player resisted the suggestion made by another

player to dump all of the tokens into the CPR by stating, "We screw ourselves, too."

24. We did not explore the effect of an external agent assigned responsibility to monitor and sanction behavior because in this research we were interested in the feasibility of internal enforcement.

25. See V. Ostrom 1987, 1989, 1991 for an elucidation of an alternative theory to Hobbes's.

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RATIONALITY, MORALITY, AND EXIT

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The morality and rationality issue is explored from an Axelrod-type perspective; that is, it is discussed in terms of recurrent-prisoner's-dilemma-type games and behavioral strategies or programs for playing them. We argue that intuitive notions of rationality and morality can be shown to be mutually compatible if two assumptions are made: (1) that morality is specified as a general behavioral disposition or program whose rationality is to be determined in comparison to alternative behavioral programs and (2) that the recurrent game is specified as a prisoner's dilemma game with an exit option. The results of a simulation experiment are presented, showing that a "moral program" (specified as one that never defects, but exits in response to an opponents defection) is successful in competition with a variety of alternative programs, including Tit for Tat.

> The reconciliation of morality with rationality is the central problem of modern moral philosophy. It is easy to achieve this reconciliation by fiat, to select a conception of rationality and a conception of morality so that the two fit. What is of greater interest and value is to attempt this reconciliation beginning with our intuitive conceptions of rationality and morality.
>
> —David Gauthier

The discussion on how rationality and morality can be systematically related to each other has a long tradition in moral philosophy. More recently efforts have been made to bring a new perspective to this issue by recasting it in terms of the prisoner's dilemma setting of game theory. The most elaborate contribution of this kind is probably David Gauthier's *Morals by Agreement* (1986), which is based on the presumption that the structure of moral problems is "represented most clearly by the Prisoner's Dilemma" and that "whatever force moral claims have derives, and derives entirely, from their role in overcoming the structural problem of interaction represented by the Prisoner's Dilemma" (Gauthier 1988, 176).

Casting the *morality and rationality issue* in terms of the prisoner's dilemma has helped to shift a debate that has often been rather elusive to a more operational level, since it requires that such attributes as morality and rationality be translated into behavioral strategies or programs (Vanberg 1988; Vanberg and Buchanan 1990). Another contribution (though not directly addressing the morality and rationality issue) of particular significance in this context is Robert Axelrod's *Evolution of Cooperation* (1984), the core part of which is a computer tournament among alternative strategies/programs for playing iterated prisoner's dilemmas.

We shall approach morality and rationality from an Axelrod-type perspective; that is, we shall discuss this issue in terms of recurrent games and behavioral strategies/programs for playing these games. More specifically, we shall argue that our intuitive notions of rationality and morality can be shown to be mutually compatible *if* appropriately specified. Two assumptions are at the core of our argument. First, we presuppose that the *rationality of morality issue* is appropriately discussed at the level of general behavioral dispositions or programs, as opposed to the level of particular actions. Second, we assume that an important aspect of what we mean by *morality* can be more adequately discussed in the context of a recurrent game that includes an *exit option*, rather than in the context of a recurrent prisoner's dilemma game that, in its pure form, rules out the possibility of exit from some or all rounds of the game.

Though the focus of this paper is on the prisoner's dilemma notion of morality, it should be noted that this interpretation is, in certain respects, both wider and narrower than our intuitive understanding of morality. As Ullmann-Margalit and Buchanan have pointed out in their respective comments on Gauthier's argument, it is wider insofar as the set of prisoner's-dilemma-type problems includes situations that we may not consider as moral problems, for example, the compliance problem faced by members of an industrial cartel (Buchanan 1988, 75–79; Ullmann-Margalit 1977, 41–42). The prisoner's dilemma notion of morality is narrower than our intuitive understanding of moral problems, since the latter includes situations that are not characterized by the strategic interdependence of the prisoner's dilemma. In other words, what we shall discuss as moral problems lies in the intersection of two sets: our intuitive notion of morality and prisoner's dilemma problems. All we need claim for our purpose is that the prisoner's dilemma notion captures a relevant aspect of morality as commonly understood.[2]

We discuss morality as a behavioral disposition and the concept of rationality as it applies to dispositions as opposed to particular acts. We look at the implications of adding an exit option to the cooperate and defect options that constitute the prisoner's dilemma and propose a definition of morality as a behavioral strategy/program, based on the two previously discussed assumptions. We present the results of a series of simulation experiments that measure the performance of a moral program in competition with a limited set of alternative programs and conclude with a brief discussion on the scope and the limits of the exit option in social interaction.

RATIONALITY AND BEHAVIORAL DISPOSITIONS

One aspect of Axelrod's tournament approach to the repeated prisoner's dilemma is its implicit shift of perspective from the level of situational choice to the level of programmed choice. By comparing alternative behavioral strategies/programs according to their relative performance over a sequence of encounters, one moves the question, What is rational? from the level of single, particular choices to the level of *programs* that provide generalized instructions for how to act in particular instances. The significance of this shift of focus from single choices to behavioral programs lies in the fact that a program may be rational in the sense of generating better overall payoffs than any feasible alternative program but may call for single actions that if considered separately, would appear irrational.

The relevant distinction here is analogous to the distinction between constitutional and subconstitutional choices in a collective choice setting. Constitutional choices are choices *among* rules/constraints; subconstitutional choices are choices *within* these rules/constraints. In a similar sense we may draw a distinction between an individual's *personal constitution* (the amalgam of that person's general dispositions and behavioral rules, or, in ordinary language, *character*) and choice behavior in particular instances. To the extent that such a distinction can be plausibly drawn, we can meaningfully separate two levels for which the question of rationality can be raised: the level of specific situational choices and the personal constitutional level. At the personal constitutional level, the relevant alternatives are not particular actions but, instead, general dispositions or behavioral programs.

Leaving aside the issue of how an individual's personal constitution is actually formed (esp. whether and to what extent it is subject to explicit choice, as opposed to unreflected behavioral conditioning), we want to suppose that our intuitive notions of morality typically refer to attributes of dispositions, rather than of particular choices. In other words, we contend that the ordinary meaning of morality is best modeled at the personal constitutional level, rather than at the level of particular situational choices.[3] We shall argue that our intuitive notion of morality can be translated into behavioral strategies that can be shown to be successful in competition with a set of alternative programs. Successful programs do not necessarily perform best in every situation, but, rather, are such that no feasible alternative program generates systematically greater payoffs. Such programs may be rationally chosen.

Readers familiar with David Gauthier's *Morals by Agreement* will notice that the purpose of our enterprise is basically the same as his. Fundamental to Gauthier's argument is the distinction between what he calls *straightforward* and *constrained* maximization—which correspond to our distinction between situational rationality and personal constitutional rationality. Straightforward maximization, in Gauthier's terminology, means to maximize (i.e., choose the most preferred alternative) at the level of particular choices (1986, 15). Constrained maximization, by contrast, means to maximize at the personal constitutional level (i.e., choose the most advantageous behavioral disposition from among potential alternative dispositions).[4]

In Gauthier's account, the morality issue is clearly a matter of general dispositions, not of single choices. Accordingly, whether morality—understood as a disposition to accept moral constraints (1986, 15)—is rational has to be judged at the level of behavioral dispositions, not at the level of particular actions (p. 182). A morally disposed person may well—and typically will—choose nonoptimally on particular occasions in the sense of not choosing what, under separate consideration of the specific case, would be the maximizing alternative. Yet this does not make the moral disposition irrational so long as no alternative disposition (even the disposition to act as a straightforward maximizer) could be expected to render systematically higher payoffs over a sequence of actions.[5]

PRISONER'S DILEMMA AND THE EXIT OPTION

In Axelrod's simulation experiments, and generally in discussions of strategy choice in recurrent prisoner's dilemma games, it is implied that the players *must play*. Players simply cannot avoid playing the game. This assumption is, of course, part of the original prisoner's dilemma story: the two prisoners can only choose between *confess* and *not confess*; they cannot exit from the situation. The restriction of choice to the alternatives, *cooperate* and *defect*, is inherent to the structure of a prisoner's dilemma game; and it is, as such, also an essential element of the structural definition of a recurrent prisoner's dilemma game. However, in the wake of its generalized reception in the social sciences, the notion of the iterated prisoner's dilemma has come to be applied to a wide range of social settings and interaction problems for which the *no exit* assumption is clearly not appropriate. And we suppose that many of the social interaction problems (to which the notion of morality is commonly applied) arise in contexts where the persons involved actually have viable exit options in the sense that they can refuse to continue to interact with their respective counterparts. We shall work out some of the implications that follow if the presence of an exit option is systematically taken into account.

To be sure, allowing players to choose whether or not to play a particular prisoner's dilemma game changes the nature of the game. In the prisoner's dilemma game with an exit option the payoff structure for the intersections of the players' cooperate and defect options remain the same. But the availability of an exit option changes the overall structure in a way that may eliminate the characteristic features of the prisoner's dilemma setting. For this very reason, the introduction of an exit option would be illegitimate if proposed as a contribution to the technical debate on the iterated prisoner's dilemma.[6] However, our principal interest is not the formal characteristics of the repeated prisoner's dilemma but the substantive relationship between rationality and morality. Our analysis is centered on the recognition that the prisoner's dilemma notion captures an essential aspect of moral problems but that the genuine repeated prisoner's dilemma, as technically defined, may not be the most appropriate model for the problem we seek to understand.[7]

The principal effect of adding the option to *refuse to play* to the strategies available in the original prisoner's dilemma is that the defect strategy may no longer be strictly dominant. What is particularly important for our purpose, is that adopting a disposition not to defect is no longer equivalent to becoming an unconditional cooperator (Figure 1).

With the inclusion of an exit option, the payoffs of exit become an important determinant of the viability of game strategies. In practice, the net benefits of exit depend on the availability of alternatives (or, more specifically, on the expected payoffs from those alternatives), whether such alternatives exist in the form of potential interactions with other players or in solitary activity.[8] In a competitive market context, for instance, the easy accessibility of alternative options decreases the opportunity cost of exit and reduces a player's dependency on any particular counterpart. In fact, one virtue of market competition lies in its tendency to reduce the cost of exit from, and consequently the dependency on, transactions with particular players. Conversely, where competition and the accessibility of alternatives is restricted, the costs of exit tend to be increased.

Figure 1 embodies the assumption that the costs and benefits of exit are such that the expected payoffs from choosing the *do not play* alternative are higher than those resulting under mutual defection but lower than those expected under mutual cooperation. The main thrust of our argument rests on this ordinal ranking of expected payoffs from mutual cooperation, mutual defection, and exit. There are, to be sure, circumstances conceivable under which the ranking of the payoffs from mutual defection and exit would be reversed. However, the potential occurrence of such circumstances does not invalidate the fundamental logic of our argument. We shall return to this issue in the concluding section.

MORALITY AS A BEHAVIORAL PROGRAM

In a short paragraph of his *Evolution of Cooperation*, Axelrod raises the question of whether Tit for Tat, in addition to being "an effective strategy for an egoist to use," is also "a moral strategy for a person . . . to follow" (1984, 136). His answer is that at least if measured against our everyday intuitions, "reciprocity does not seem to be the height of morality" (p. 137). He suggests, however, that Tit for Tat may well be the best we can hope for in moral affairs. Principles like the Golden Rule or the Kantian categorical imperative (which are widely regarded as exemplary moral standards) seem to require us to adopt the nonviable strategy of unconditional cooperation. Such a strategy not only is detrimental to the person adopting it but also "tends to spoil the other player" (p. 136) and thus allows noncooperative, exploitative programs to flourish. A disposition to cooperate unconditionally is not protected against exploitative programs and will tend to be selected against in a competitive, evolutionary environment. Furthermore, it not only promotes its own elimination but is also detrimental to other "friendly" programs because it nourishes exploitative programs. By contrast, Tit for Tat does well for itself and is actively supportive of a moral environment. It rewards cooperation and punishes defection. Therefore, Axelrod concludes, although its eye-for-an-eye message gives reciprocity a "slightly unsavory taste," "a moral person couldn't do much better" (p. 137).

If, as Axelrod suspects, Tit for Tat seems somewhat at odds with our intuitive notion of morality, the reason probably lies in the fact that *defection* is included in its behavioral repertoire. When we think of defection as lying, not keeping promises, defrauding, stealing, exploiting, and so on, it seems, indeed, difficult to attach the attribute *moral* to a behavioral program that includes such options even if they are exhibited only in response to an opponent's prior defection.[9] One obvious problem with such responses in kind is that they send potentially ambiguous signals to opponents and (perhaps more importantly) to third parties. They send ambiguous signals in all cases where it may not be totally obvious to opponents and onlookers whether the defection is a retaliatory act of an otherwise moral program or simply defection of an exploitative program. Since behavioral programs or dispositions are not directly observable but can only be inferred from observed responses in particular situations, any use of defection options like the aforementioned examples raises the information costs for deciphering what kind of program is behind a person's observed actions. This, in turn, may invite attempts to camouflage as retaliatory sanctions what are, in fact, exploitative defections.

If morality as a behavioral program is not to be doomed from the outset, it can only be a program of *conditional* cooperation, that is, a program that dic-

FIGURE 1.

Payoffs for a Prisoner's-Dilemma-with-Exit Game

		Individual 2		
		Cooperate	Defect	Withdraw
Individual 1	Cooperate	R, R	S, T	N, N
	Defect	T, S	P, P	N, N
	Withdraw	N, N	N, N	N, N

$T > R > N > P > S$

tates cooperation with cooperators but at the same time includes protection against exploiters. The willingness to cooperate can be specified as a disposition never to be the first to defect, while the ability to protect against exploiters can be seen to rest in a disposition not to continue to cooperate when an opponent defects. There are three varieties of conditional cooperative programs: Tit for Tat, *prudent morality, retributive morality*. The latter two differ from Tit for Tat in that they exclude the defection option. This feature, we suppose, is an essential ingredient of our common notions of morality.

Retributive morality is a program that excludes the defect option from its behavioral repertoire and, rather than responding in kind (like Tit for Tat), responds to an opponent's defection by some explicit punishment that typically involves some costs to the actor. The cost feature tends to eliminate the ambiguity attached to Tit for Tat's punishment by defection. Because of its less ambiguous nature, retributive morality is potentially more effective than Tit for Tat in supporting "general" morality in a community. It shares, however, to some extent, the problematic feature of Tit for Tat that it can get caught in a vicious cycle of mutual retaliation once a feud gets started (Axelrod 1984, 138).

We shall concentrate attention on prudent morality. Under a program of prudent morality, an individual cooperates whenever he chooses to play the game but refuses to play with individuals who have defected in the past. Prudent morality is similar to retributive morality in that it excludes the defect option from its behavioral repertoire. Its difference from the latter lies in its more defensive and less corrective nature. Retributive morality requires the actor to take action explicitly to punish a defector, whereas prudent morality simply dictates refusal to continue to deal with the opponent.

Prudent morality contributes principally to one of the two practices that support the moral order of a social community. It dictates strict compliance with moral rules; but because it fails to impose explicit punishments on defectors, it only indirectly deters others from defecting. The refusal to continue to interact with a defector is, of course, a punishment of sorts; but it tends to be less deterring than the retaliatory responses of Tit for Tat or retributive morality.[10] Yet even if its laxity in punishing defectors may be seen as a flaw, prudent morality seems to us to be, among the strategies of conditional cooperation considered here, overall, the least controversial candidate for a program representation of what is commonly understood as morality.[11] The fact that it is defined by what it *does not do* (i.e., defect) also seems in line with the *thou shalt not* terms in which moral principles are often stated. We, therefore, include prudent morality as the *moral program* in the simulation experiment we shall discuss.

Our discussion now allows us to restate the issue of the relation between rationality and morality in a fashion to satisfy David Gauthier's demands. If we specify the concept of morality suggested here and we define rationality as choosing from among a set of alternatives the one that generates the highest payoffs (or, at least, payoffs not systematically below those of any feasible alternative), we not only use these concepts compatibly with their common understanding, but we also use them in a sense that makes them definitionally independent. Whether rationality and morality, so defined, are compatible is a *factual*, not a definitional, matter. Whether morality is compatible with rationality has to be answered in the light of a comparison between the overall payoffs that actors can expect from potential alternative dispositions or behavioral programs. To show the rationality of morality would mean to demonstrate that in competition with alternative behavioral programs, the moral program is more (or, at least, no less) successful than any other and, consequently, a program that can be rationally chosen.[12]

THE VIABILITY OF ALTERNATIVE DECISION RULES

The rationality to an individual player of alternative dispositions is essentially a matter of the cumulative payoffs generated by such programs in the community where an individual is located. Since, as Axelrod (1984) has shown, cumulative payoffs vary with the community of players with whom an individual player (or program) interacts, individuals are likely to find different strategic programs rational in different settings. To demonstrate that a particular program has general appeal, it is necessary to show that it performs, on average, no worse than any other. We consider two programmatic implementions of prudent morality—one intolerant, one more tolerant. The difference between the intolerant and the tolerant version is that the former terminates all interaction after a fellow player's first defection while the latter gives a defecting opponent a second chance after withdrawing for one round. The intolerant program is examined first in as much as it is the simpler program and has properties identical to the tolerant program in the first several tournaments analyzed.

All of the tournaments use the same payoff matrix for each prisoner's-dilemma-with-exit encounter between individuals in the communities of interest. The payoffs are (1,1) for the *cooperate, cooperate* cell and (−1,−1) for the *defect, defect* cell. If either player chooses not to play a particular game, the payoffs are (0,0). In the exploitative off-diagonal case, the player who cooperates receives −2 while the player who defects receives 2. The same payoff structure has been used for illustrative purposes by Tullock (1985).[13]

Elementary Strategies with Exogenous Community Populations

We shall report simulation results from two series of tournaments involving four decision rules in an environment where players confront prisoner's dilemma games with an exit option (PDE), then analyze settings where the population of players is endogenously determined by payoffs in previous rounds of the game. In the first series of tournaments examined, the number of PDE pairings among individuals is varied within an exogenously determined population of four elementary strategies. We find that the prudent moral strategy does best in an environment where many pairings occur. In the second series of tournaments, both the number of PDE pairings and the composition of the relevant group is varied. This series of tournaments demonstrates that advantage of prudent morality is greatest in relatively harsh settings. The prudent strategy is more robust than the other four programs in the sense that its performance is relatively more independent of the strategic programs used by other players in the community.

In each pairing with another player, individuals are assumed to use a particular strategic program that determines whether or not they participate in the prisoner's dilemma game before them and, if so, whether they cooperate with the other player or not. Individuals are characterized by their strategic programs. In the first round of play, individuals know nothing about their counterparts. In subsequent pairings, they remember whether particular players defected in previous rounds of play or not. Of course, only the adaptive strategies make use of this information. The payoffs for each encounter are those we have specified.

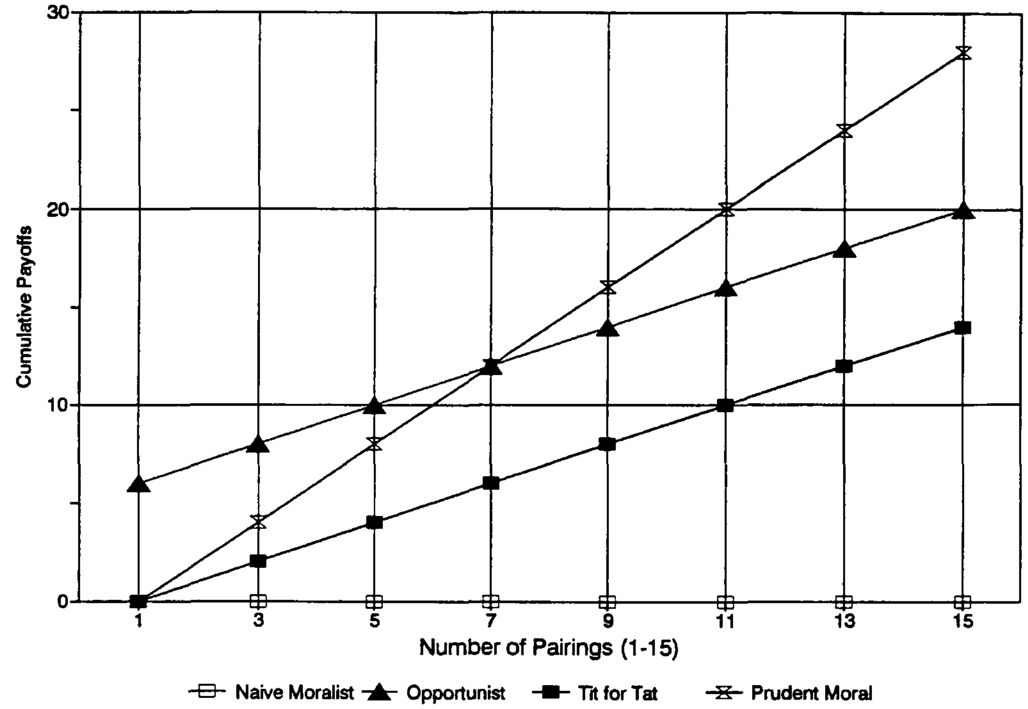
To make the results comparable to Axelrod's tournaments, we assume that several of the players have adopted dispositions that always require playing the game at hand. This allows the advantage of strategies that make use of an exit option to be readily demonstrated. Initially, our attention is focused on four strategic programs: (1) always play and always defect, the *Opportunistic* strategy; (2) always play and always cooperate, the *Naive Moralist's* strategy; (3) always play and use the strategy that the other player used the last time they met, *Tit for Tat*; and (4) play only with those who have never previously defected but always cooperate if one chooses to play, the *Intolerant Prudent Moral* strategy. All but the opportunistic strategies are "nice" strategies, in that they play and cooperate in the first round of play with a stranger. The first two programs are implemented as if they are dominant pure strategies, inasmuch as the course of play is not affected by past rounds of the game. The last three programs are adaptive strategies in that previous rounds of play affect their current behavior vis-à-vis their counterparts.

As in Axelrod's work, our simulations clearly reveal that the viability of a particular strategy varies with the strategies used by other players and with the number of times players confront each other in PDE games. However, in contrast to Axelrod's work, we find the Tit for Tat strategy to be less robust than the selective strategy of the prudent moralist.

The first series of tournaments shows that the viability of alternative dispositions varies with the

FIGURE 2.

1–15 Pairings, Cumulative Payoffs



number of times that individuals confront each other in PDE settings. Figure 2 summarizes the cumulative scores realized in several series of pairings between four individuals, each playing one of the four strategies described. They do not meet themselves. At the left-hand side of the chart, players meet each other a single time in the PDE setting. In this case, the opportunistic strategy is substantially more viable than any of the others. It yields a payoff of 2 in each of the pairings with the other players and thus a total score of 6. The other strategies generate a score of 0—a payoff of −2 in their pairing with the opportunist and payoffs of 1 with each of the other two players. Note that as the number of pairings increases, the viability of both Tit for Tat and the prudent moral strategy increases. Both adaptive strategies reduce losses from subsequent encounters with opportunistic players. Tit for Tat reduces them from −2 to −1. The prudent moral strategy reduces them from −2 to 0. Because of this greater reduction in losses from opportunistic players, prudent morality dominates Tit for Tat in two or more encounter games. Moreover, it outscores the opportunistic strategy in settings where players meet each other in PDE settings seven or more times. Tit for Tat never outscores the opportunistic strategy in this population of players.

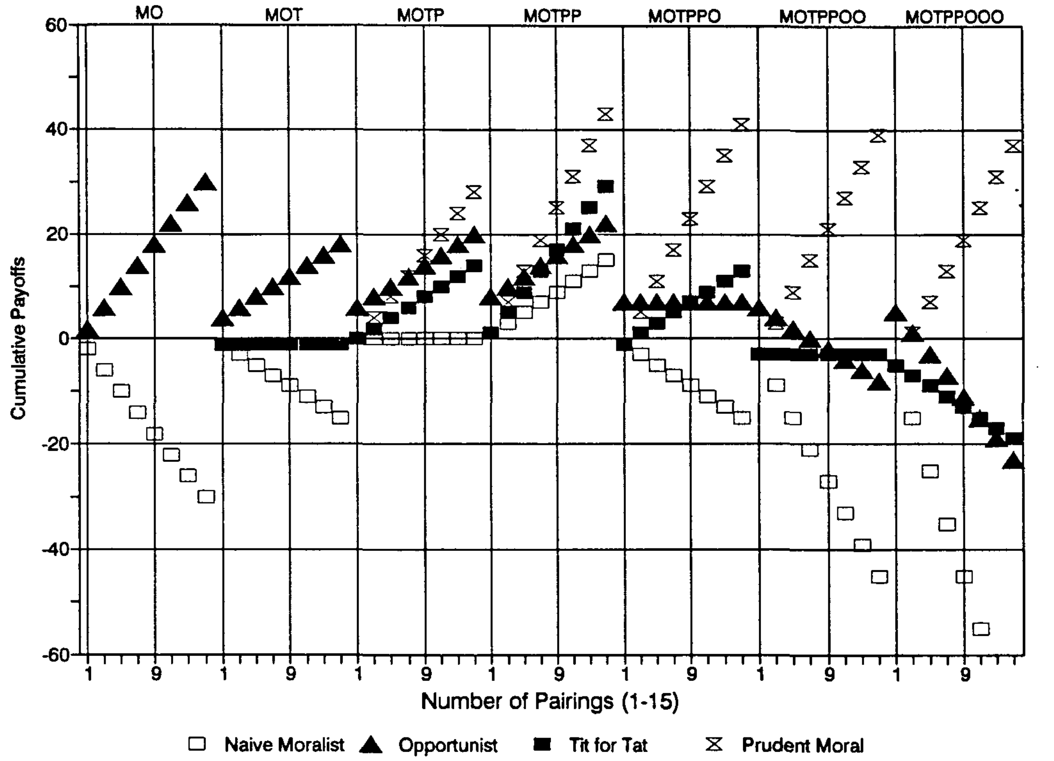
The second series of tournaments analyzes the extent to which the four strategic programs are affected by changes in the population of players confronted. The results clearly demonstrate that the attractiveness of a particular strategic program varies with the population of players faced. However, the results indicate that prudent morality is the least sensitive to community composition and scores uniformly better than the other programs if there are fairly numerous pairings with other players.

Figure 3 summarizes the cumulative scores realized by individuals playing the four strategies in seven different communities composed of different numbers of players. In each community, a series of tournaments similar to those reported in Figure 2 is conducted, in which pairings between players vary from 1 to 15. Figure 3 thus summarizes the results of 105 different simulations. Community composition is identified by a series of letters along the top of the figure. The various numbers of pairings between players within each community are written along the bottom of the table. The vertical dimension represents cumulative payoffs. Each symbol (triangle, rectangle, etc.) characterizes the payoff of a particular strategy. The location of each symbol represents the cumulative score that an individual playing that strategy has realized within the relevant community after a particular number of pairings with each of the other players.

The first community, at the far left of the diagram, consists of a single naive moralist (M) and an opportunist (O) and is labelled MO. In this setting the opportunistic strategy easily dominates the moralist strategy for all ranges of encounters. The second community adds a Tit for Tat player (T) to the previous community and is characterized by the subinterval labeled MOT. Note that the addition of

FIGURE 3.

Payoffs in Alternative Populations



the Tit for Tat player enhances the welfare of the naive moralist, while it reduces the score of the opportunist at all levels of encounters. However, the opportunistic strategy continues to be the most viable. The third community is identical to the one analyzed in Figure 2; it adds a prudent moralist (P) to the previous group and is labeled MOTP. Note that inclusion of a prudent moralist improves the lot of all the other players at all encounter levels.

The fourth community adds another prudent moralist to the community (MOTPP). Note that the addition of a second prudent moralist again increases the welfare of all other community members and makes the prudent moral strategy even more viable over a wider range of encounters. It now dominates the opportunistic strategy in settings where at least five PDE games are played between each pairing of the five community residents. In this environment, Tit for Tat emerges as the second most viable strategy in settings where PDE games are relatively numerous. Even the naive moral strategy is viable in this setting in the sense that it yields, for the first time, a positive net score over the entire range of possible encounters. The viability of the naive moral strategy requires a relatively large number of other benign players.

Communities 5, 6, and 7 show the effects of adding one, two, and three additional opportunistic players to the fourth community (yielding MOTPPO, MOTPPOO, and MOTPPOOO). Note that the addition of other opportunistic players worsens the lot of all other players, including other opportunists. The scores of the prudent moral strategy are the least affected and now exceed the opportunistic strategy over a wider range than in the earlier communities. In the rightmost community (MOTPPOOO), the prudent moral strategy scores better than any other if there are two or more PDE pairings with other community residents. It is the only viable strategy if there are four or more encounters in the sense that only the prudent moral strategies yield positive scores in this range. The naive moralist's strategy is by far the worst strategy in this relatively nasty community. Although gains are made from dealings with the two prudent moralists and the Tit for Tat player, losses generated by interactions with opportunistic players far outweigh those gains.

The second tournament demonstrates that the prudent moral strategy is highly viable in many settings and that communities with a relatively large number of such players tend to be relatively more successful than communities composed of a relatively large number of opportunistic players. Indeed in relatively inhospitable communities like community 7, the prudent moral strategy was the *only* viable strategy if PDE pairings are frequent.

All this suggests that individuals who adopt a prudent moral strategy will be relatively more successful than those who adopt other programs of similar complexity, within this range of environments. On the other hand, each of the other programs is viable in the proper context. To explore whether the prudent moralist's strategy has an evolutionary advantage, another series of tournaments is analyzed, in which community composition is determined endogenously.

Tournaments with Endogenous Population Mixes

In the third series of tournaments, community composition is endogenously determined by past cumulative payoffs. Here we find that prudent morality is generally the most successful of the strategies in the tournament, insofar as it scores well and therefore tends to be more than proportionately represented in viable communities. We examine two types of evolutionary simulations but report in detail only the more interesting one. Simple population growth models yield relatively unsurprising results. In the case where community size is allowed to grow and the number of PDE encounters is greater than five, individuals who adopt the prudent moral strategy rapidly displace all other player types. In settings with a large number of pairings, the prudent moral strategy yields the largest cumulative payoffs and grows most rapidly. This is evident from the results reported in Figure 3. In cases where the number of encounters is small, opportunistic players tend to displace other types of players. However, the resulting community disappears because a community of opportunists is not viable. In this sense, natural selection might be said to favor players who adopt the prudent moral strategy.

A somewhat more rigorous test of viability of alternative dispositions involves competition within a population of fixed size. These simulations reveal that in cases where the community size is held constant but its composition is allowed to vary according to past cumulative scores, community composition fluctuates in an irregular cyclic manner. This fluctuation occurs in part because we allow strategic programs to regain popularity if the social environment changes so as to increase the viability of those programs. Insofar as moral decision rules reflect social, rather than genetic, conditions, total elimination of a particular rule is a less plausible evolutionary result than selective transmission and rebirth. Rather than disappear forever, unsuccessful decision rules may be expected to reappear if social circumstances change in a way that increases their viability.

The tournaments involve communities in which 25 PDE games are played with players other than themselves. Initially, the dispositions of players are equally distributed throughout the community; but in succeeding periods, they are proportional to positive scores generated in the previous rounds. Programs that generate negative or zero scores in a particular generation of the community disappear for at least one generation. Programs *reappear* if the community's composition becomes more favorable at some point in the future. To allow this possibility, the score that missing strategies would have generated had they been present is used to determine its relative size or population share in the next generation. In the first evolutionary tournament, the population shares sum to four in each period, reflecting the constant population assumed for this series of simulations.[14]

Figure 4 reports the results of such an evolutionary tournament between the four dispositions used in the other series of tournaments. Population shares are plotted in a stacked diagram. Vertical distances between lines represent the relative size of the various decision rules. The shaded areas highlight the time path of population shares of specific strategies. The average width gives an indication of the relative success of the various strategies over the time period of interest. Note that the time paths of population shares for both nonadaptive strategies are highly cyclic in nature. Both the naive moralist and opportunistic strategies occasionally disappear entirely. For example, the naive moralist strategy disappears in the second generation. This, in turn, makes the opportunistic strategy less viable, since it does poorly in a community composed of other opportunists, Tit for Tat players, and prudent moralists. With the disappearance of the opportunists, the naive moralist strategy returns to viability and reappears in the third generation. This, of course, makes opportunism a more viable strategy—and so on. The population share of Tit for Tat is relatively stable, ranging between .86 and 1.33. The population share of players using the prudent moralist strategy is also cyclic but never falls below 1 and exceeds 2 at some point during each cycle. If population share is used as an index of viability, the prudent moral strategy is the most viable of the four programs in this tournament. As indicated by the previous series of tournaments, the prudent moral strategy does well in most communities.

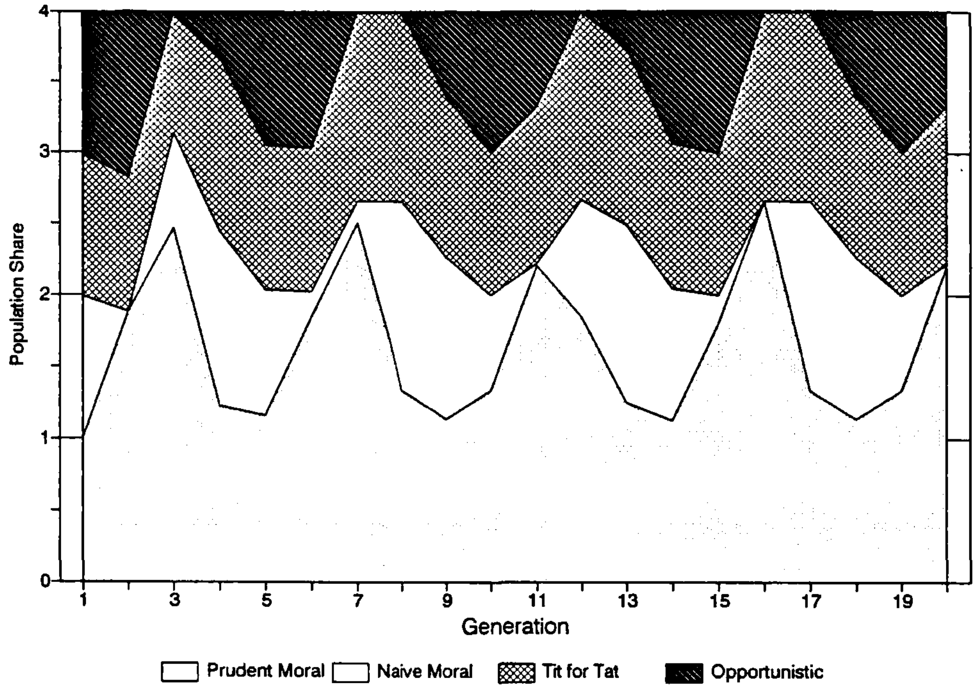
Evolutionary Tournaments with More Sophisticated Strategies

In the last series of tournaments, several more sophisticated strategies are added to the repertoire of possible programs. We first consider a fifth strategy, *Hit-and-Run*, a prudent form of opportunism. Hit-and-run is added to determine whether the exit option itself, rather than the moral program, was the source of prudent morality's superior performance. The Hit-and-Run strategy is the mirror image of the Prudent moral strategy in that it calls for playing only with individuals who did not defect in the previous period. However, whenever the game is played, the Hit-and-Run player defects.

We follow the analysis of Hit-and-Run with consideration of sixth and seventh strategies, *Tester* and *Tolerant Morality*. The Tester strategy tests to see whether or not other groups of players are exploitable naive moral types who always cooperate. It defects in the first meeting with each other player and

FIGURE 4.

Evolutionary Success (25 Pairings)



continues to defect as long as the other player cooperates. If the other player does not cooperate, Tester cooperates twice and thereafter plays tit for tat. Tester is designed to make Tit for Tat score relatively better by responding to nonexploitability with two cooperative rounds (rather than one).[15] Tester may be considered a trainable program insofar as retribution via defection or exit transforms tester from an exploitative strategy into a well-behaved Tit for Tat player.

The Tolerant Moral strategy shares with the intolerant Prudent Moral program the property that it never defects. It is more tolerant in that it does not give up on an opponent after a single defection. Instead, it responds to defection with a one-period exit followed by cooperation. If a second defection is encountered, it withdraws from all future PDE games with such players. More sophisticated versions of Tester and Tolerant Morality are possible, but these programs capture essential elements of a wide array of reactant and tolerant strategies.

In the first of the tournaments including more sophisticated strategies, we explore whether or not the advantage of the prudent moral strategy is simply a result of the fact that it is the *only* program using the exit option. To shed light on this question, the Hit-and-Run program is added to the previous set. Its advantage over the nonadaptive opportunistic strategy is that it avoids long-term unproductive engagements with Tit for Tat players and other opportunistic programs.

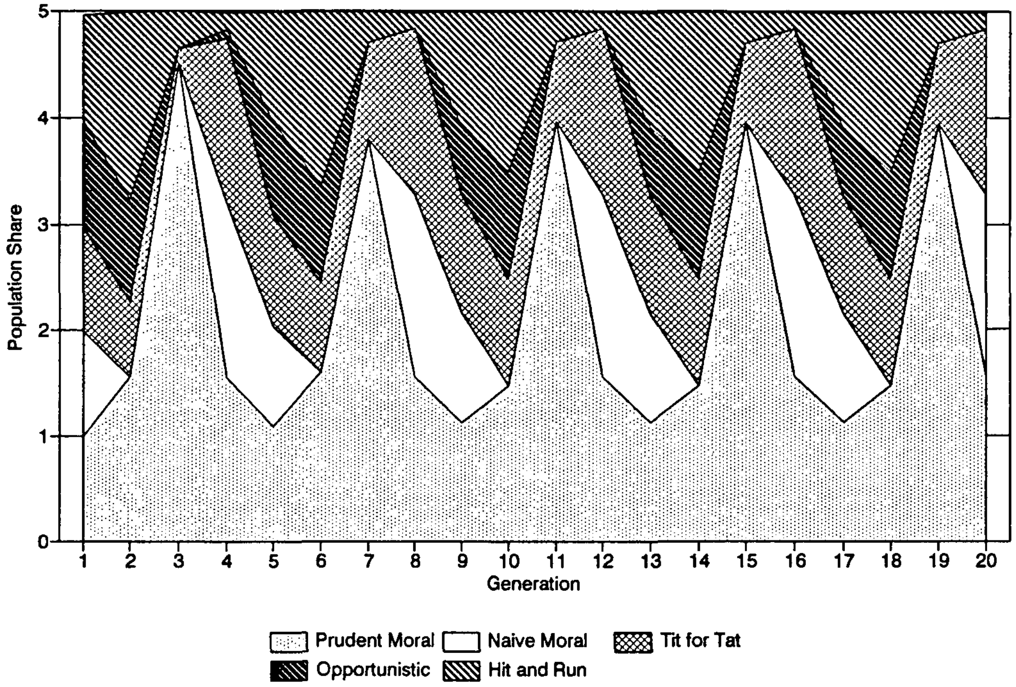
Figure 5 reports the results of an evolutionary tournament for the resulting population of five strategic programs. The results are similar to those of the previous tournament between four programs. Again, the cyclic pattern occurs; and again, the Prudent Moral program proves to be on average the most successful. The Hit-and-Run strategy is more viable than the two naive strategies in that it never completely disappears. However, its population share is never larger than the Prudent Moral strategy and is often smaller than that associated with Tit for Tat.

In the last series of tournaments reported here, the Tester and Tolerant Morality programs are added to the previous population of five strategies. Tester is an opportunistic, but trainable, version of Tit for Tat. The Tolerant Moral program is similar to the Prudent Moral program except that it checks to see whether the behavior of players who have transgressed (defected) in a previous round of the game might be altered by exit. If a second defection occurs, it ceases to interact with those using the offending strategy.

The results of this tournament are reported numerically in Table 1. The numbers represent population shares in a community of seven strategies and are similar to those plotted for the previous evolutionary tournaments. The results are broadly similar to those of the previous tournaments in that there are cycles and the moral strategies do well. However, there are several new results of interest, the first of which is that with the addition of two more retributive strategies, the nasty strategies do significantly worse in this tournament than in the previous ones. The simple opportunistic strategy disappears in round 2

FIGURE 5.

Evolution (5 Strategies, 25 Pairings)



and never returns, while the Hit-and-Run strategy shrinks to a range between .1 and .3 reflecting variation in the small pool of naive moralists available to exploit. Interactions with Tit for Tat and Tester, together with shunning by the two moral programs, combine to make the simple opportunistic strategy inviable in this setting. Hit-and-Run's participation in games with programs other than the naive moralist cease after the second meeting with other programs.

The four most viable strategies in this setting are Tit for Tat, the original (intolerant) Prudent Moral program, Tester, and the Tolerant Moral program. Tester and the original Prudent Morality program do significantly less well than Tit for Tat and the Tolerant Moral programs. After the first round of the game, Prudent Morality ceases to interact with Tester, which reduces the cumulative scores of each. Note that in the first round of the game, where there are still relatively large numbers of individuals using the Opportunistic and Hit-and-Run strategies, the two moral strategies are more successful than either Tester or Tit for Tat. In relatively nasty environments, the opportunity cost of exit is low—in fact, negative.[16] With the disappearance of the simple Opportunistic program in the second round, the relative performance of the original intolerant program suffers, and Tit for Tat gradually converges to about the same population share as the Tolerant Moral program. In this part of the game, interactions with Tester (or the lack thereof) are crucial. Both the Tit for Tat player and the Tolerant Moralist are able to "persuade" Tester to give up his exploitative behavior with respect to themselves, although not for others. This provides them with another useful partner in PDE settings. Here, the opportunity costs of exit are significant and reduce the long-term performance of the intolerant form of Prudent Morality. The original intolerant Prudent Moralist does not check to see whether defectors are like Tester, who might be "taught a lesson," and thereby misses profitable interactions with Tester. On the other hand, in more dangerous settings like those of the first round, where a large number of unteachable defectors exist, not giving a defector a second chance is the second most viable strategy.

All in all, the simulation results suggest that a strategy of cooperating with those who have cooperated in the past and avoiding those who have defected in the past is eminently viable and is robust over a wide range of community compositions. The advantage of tolerant versions of this strategy depends on the population mix. Tolerance has advantages if there are other programs in the community whose behavior can be changed by imposing modest sanctions.[17] Overall, the prudent moral strategies are viable moralities. Moreover, the results are only somewhat sensitive to the magnitude of the payoffs. For example, a doubling of the off-diagonal payoffs has little effect on the evolutionary simulations, although it does increase the scores of the opportunistic strategy in settings with relatively few encounters.[18]

Table 1.

Evolution within a Fixed Population (7 Initial Programs, 20 Generations)

GENERATION	NAIVE MORAL	OPPORTUNIST	TIT FOR TAT	INTOL. MORAL	HIT-AND-RUN	TESTER	TOL. MORAL
1	1	1	1	1	1	1	1
2	0	.27	1.292	1.331	1.022	1.35	1.736
3	0	0	1.94	1.493	.11	1.384	2.072
4	.775	0	1.741	1.351	.106	1.284	1.743
5	.52	0	1.611	1.277	.526	1.454	1.613
6	.021	0	1.803	1.345	.451	1.568	1.811
7	0	0	1.97	1.435	.123	1.496	1.977
8	.677	0	1.775	1.342	.103	1.325	1.777
9	.496	0	1.641	1.289	.48	1.453	1.642
10	.069	0	1.798	1.345	.431	1.551	1.805
11	.019	0	1.947	1.426	.157	1.497	1.954
12	.346	0	1.863	1.406	.12	1.399	1.865
13	.445	0	1.74	1.345	.311	1.418	1.742
14	.264	0	1.762	1.342	.382	1.485	1.766
15	.138	0	1.85	1.381	.282	1.495	1.855
16	.229	0	1.862	1.396	.198	1.45	1.866
17	.344	0	1.801	1.371	.25	1.431	1.804
18	.31	0	1.778	1.355	.319	1.457	1.781
19	.225	0	1.811	1.367	.305	1.477	1.815
20	.222	0	1.838	1.381	.253	1.465	1.842

Note: We report population shares of alternative behavioral programs in successive generations. Scores are calculated for 25 rounds of play between each pairing of rules. These pair scores are then weighted by population shares and summed to give generational payoffs for each program. Population weights are determined by the relative payoffs of program interactions in the previous generation.

SCOPE AND LIMITS OF EXIT

A venerated tradition in philosophy centers around the axiomatic assumption that morality, by necessity, means self-sacrifice and that consequently, the teaching of morality is essentially the teaching of self-denial.[19] For this tradition, self-sacrifice is by definition an attribute of morality; and it follows as an implication from such a definition that morality cannot be a rational disposition in the sense of furthering the well-being of the person adopting it. The results of our discussion support the equally long-standing tradition of critical arguments against such understanding of morality. It shows that the basic thrust of our common understanding of morality and rationality can be specified in a fashion that makes the two definitionally independent; and it also shows that the two independently defined notions are, in fact, compatible. In other words, it shows that there is no reason to assume that morality is necessarily self-sacrificial unless we insist on defining morality in a way that rules out the possibility of morality's ever being rational. Those who insist on the self-sacrifice notion of morality may rightfully have in mind that a moral disposition implies the adoption of constraints on one's own choice of behavioral strategies. What they fail to recognize is that such constraints need not be in conflict with self-interest. As David Gauthier puts it, constraints on the pursuit of one's interest can well "have themselves a foundation in the interest they constrain" and can "make possible the more effective realization of one's interests" (1986, 2, 103).

To be sure, the assumption that an exit option is readily available (i.e., that the costs of exit are relatively low) plays a critical role in our argument. In cases where exit costs are low, the prisoner's-dilemma nature of the "iterated prisoner's dilemma" essentially disappears. Where player A has access to alternatives that promise payoffs close to what he could expect from mutual cooperation in an ongoing game with player B (as in a competitive market), exit is essentially costless. Interaction with defectors can be easily avoided, and defectors will find that they are avoided.[20]

With a low-cost exit, since a defecting counterpart can be easily avoided, there is little reason for a player to make efforts to change the opponent's behavior. To the extent, however, that attractive alternatives are not easily available, efforts to "correct" a defecting opponent's behavior may become preferable to leaving the interaction. Where the payoffs that player A can expect from mutual cooperation with player B are significantly higher than what A expects from the next best alternative, A will have an interest in undertaking efforts to "correct" a defecting B's behavior. One can apply A. O. Hirschman's (1970) well-known distinction between *exit* and *voice*. The higher the costs of exit from a particular counterpart, the more important is the use of voice, in the sense of efforts to improve the relationship.

It can be concluded from this argument that prudent morality (the disposition never to defect and to restrict one's choice options to the alternatives *cooperate* or *do not play*) becomes an increasingly more viable program as the costs of exit from a given setting decrease. With decreasing exit costs, the prospects for future gains become less and less reliant on efforts to change a defecting opponent's behavior. It is under such conditions that the two other strategies of conditional cooperation, Tit for Tat and retributive morality, become less attractive candidates for a viable morality. On the other hand, as the costs of exit increase, the role of voice, (i.e., efforts to influence a particular counterpart's behavior) gains in importance, compared to exit. We cannot easily exit from our neighborhood; the investments that we have made in friendship relations rule out easy substitutability. In relations like marriage we deliberately raise exit costs in order to make commitments credible so as to generate rewards that otherwise would be unattainable. This suggests that in such relations, Tit for Tat and retribution may play a much more significant role than in market relations.[21]

If this argument on the relation between exit costs and the adequacy of the different versions of a conditionally cooperative strategy is correct, one should expect this to be reflected in our common notions of morality. And there seems to be, indeed, some evidence suggesting that our intuitions of what is moral vary with relevant differences in exit costs. At the high-cost end of the spectrum, moral justification for Tit for Tat and retributive behavior seem to be fairly common, whereas Prudent Morality gains in importance as we move to the low-cost end. Another implication is that contrary to a common perception, markets are not parasitic on moral attitudes that have to rely, for their nourishment, on other social settings. Instead, the more the essential feature of markets (coordination by voluntary contract) is applicable and secured, the more markets reinforce the virtues of honest behavior that in turn, make them work effectively. In fact, one of the major defects of socialism may be seen in the fact that it is unable to create and nourish the moral attitudes that it would need to be workable.

Notes

We are indebted to Geoffrey Brennan, James M. Buchanan, Mark Crain, and Hartmut Kliemt for helpful comments and suggestions on earlier versions. A previous draft of the paper was presented at the Public Choice Society meetings, New Orleans, 1991.

1. To be sure, one could also argue that moral problems as such are defined relative to some community and that, in this sense, for the community of cartel members, the problem is a moral one. Such a broader interpretation would seem to be implied, for instance, in D. T. Campbell's interpretation of moral norms as "our individually rational preferences as to how others behave" (1986, 171).

2. It should be noted that our discussion is also limited in the sense that it deals only with moral problems that are (or can be disaggregated into) two-person prisoner's dilemmas. The relevance of the distinction between two-person prisoner's dilemmas and N-person prisoner's dilemmas for the study of moral rules is discussed in Vanberg and Buchanan 1990.

3. In his 1965 essay, "Ethical Rules, Expected Values, and Large Numbers," J. M. Buchanan has argued along similar lines: "Note that the choice to be analyzed is that between separate *rules* for behavior, not that between separate *acts* in particular circumstances. . . . The individual is presumed to be facing the following question: What ethical rule shall I adopt as a guide to my behavior in subsequent actions? There are two alternatives before him. He can adopt a rule which we shall call the moral law, or he can adopt a rule which, loosely, we shall call the private maxim. By selecting the first, the individual commits himself to act in subsequent situations on the basis of something like the generalization principle. . . . By selecting the second instead, he commits himself in advance to no particular principle of behavior. He retains full freedom to act on the basis of expedient consideration in each particular situation that arises" (1977).

4. David Gauthier writes, "A disposition is rational if and only if an actor holding it can expect his choices to yield no less utility than the choices he would make were he to hold any alternative disposition" (1986, 182–83).

5. This is not to argue that moral rules necessarily replicate the series of actions that would have occurred in a perfectly informed maximizing strategy. If the disposition always—in each and every instance—to maximize perfectly were a feasible program, then the person, for obvious reasons, could not do better than adopt this program. However, for imperfect agents (i.e., agents who make mistakes), reliable long-term case-by-case maximization is simply not a feasible program (Heiner 1990). Our analysis shows that relatively simple moral rules perform at least as well as comparable pragmatic rules.

6. It has been occasionally observed in discussions on the iterated prisoner's dilemma that the presence of an exit option makes a critical difference (see, e.g., McLean, Orbell, and Daws n.d., Orbell, Schwartz-Shea, and Simmons 1984; Schuessler 1989; Witt 1986). Indeed, the inclusion of an exit option may change the nature of the game such that we are, in effect, no longer dealing with a genuine iterated prisoner's dilemma. To be sure, one could argue that the iterated prisoner's dilemma itself is also no longer a true prisoner's dilemma. However, while in an ordinary iterated prisoner's dilemma, the single constituent games are prisoner's dilemmas if considered separately, this is no longer true for the prisoner's dilemma with exit.

7. In his article "Adam Smith and the Prisoner's Dilemma," Gordon Tullock (1985) noted that interaction between firms and consumers in ordinary competitive markets is typically not analogous to an iterated prisoner's dilemma because actors are free to "select their opposite numbers." He suggests that such market interaction can be more appropriately illustrated in a setting where *self-selected* pairs of players can play an iterated prisoner's dilemma (on computer terminals), with each player retaining the option at any time to terminate an ongoing game (or exit the booth) and seek out some other willing partner, so that, in effect, "the people who play the games do so in voluntarily chosen pairs" (p. 1074). In such a setting, Tullock argues, "the prisoners' dilemma vanishes because the individual players have a strong desire to establish credibility so that they can play in future games. The basic reason is simply that people voluntarily choose their own partners" (ibid.).

8. We shall specify further on what, exactly, is meant by *exit*—in particular, whether it means a *definite* or a *temporary* refusal to interact with a defector.

9. When interacting with its own kind or with unconditional cooperation, Tit for Tat is, in fact, indistinguishable from unconditional cooperation: it always cooperates and never defects. In this sense, the success of Tit for Tat in Axelrod's simulation experiments shows that a rational egoist, under appropriate conditions (i.e., when interacting with suitable opponents), will behave morally. It does not show that morality is rational.

10. To be sure, a total termination of interaction may be more damaging than a single retaliatory defection.

11. It is useful to take a brief look at the relation between our concept of prudent morality and David Gauthier's notion of *constrained maximization*. To our knowledge, Gauthier nowhere really specifies constrained maximization in terms of

an explicit behavioral program. It is obvious that he wants it to be different from unconditional cooperation. "A constrained maximizer," he argues, "makes reasonably certain that she is among like-disposed persons before she actually constrains her direct pursuit of maximum utility" (1986, 169). And he emphasizes that "persons disposed to co-operation only act co-operatively with those whom they suppose to be similarly disposed" (p. 172). But it is less clear whether he wants generally to exclude the defect option from a constrained maximizer's behavioral repertoire or not; nor is it quite clear which role, if any, he sees for retribution (see Buchanan 1988, 81–82). We think, however, that an attempt to translate the thrust of Gauthier's argument into a behavioral program should generate something similar to our notion of prudent morality. If, indeed, there is a systematic difference between his notion and ours, it probably lies in the fact that he seeks to specify morality in terms of a *disposition to cooperate*, while our emphasis is on morality as a *disposition not to defect*. We suppose that the latter notion lends itself to a more straightforward and unambiguous specification as a behavioral program.

12. It should be noted, however, that such rationality of morality does not eliminate the problem of *temptation* at the moment of choice. The issue (briefly mentioned earlier), of how behavioral dispositions or programs are formed or adopted, is of particular relevance in this context. Situational temptation is apparently less of a problem for tacitly conditioned dispositions than for deliberately and rationally adopted programs. In fact, whether (from the perspective of a strict rational choice model) an actor can be assumed able effectively to commit to a rule is an interesting issue.

13. In terms of the letters used in Figure 1, the payoffs are: $T = 2$, $R = 1$, $N = 0$, $P = -1$, $S = -2$.

14. The share of the ith strategy going to population W_i is determined by the formula

$$W_{it} = 4S_{it-1} / \sum_{i=1}^{N} S_{it-1},$$

where S_{it-1} is the cumulative score realized in the previous round of the game by strategy i. The population shares sum to N, the number of programs considered. The use of fractional population shares allow the results to extend to arbitrarily large, stable populations. Since a program cannot have a negative population share, programs that generated a negative score in the previous round are given a population share of zero and excluded from the denominator of the weighting formula. If a strategy was dropped from the previous round, next period's population share is determined by the cumulative payoff that the strategy program would have realized in the previous round of play had it been included. The lack of an inertia factor makes the time path more jagged than it would have been and is best interpreted as movement from one long-run equilibrium to another. This is a fairly severe test for a strategy, since its gains can be lost overnight, as the population of strategy programs changes. The cumulative score of strategy i in generation t is calculated by determining the score that such a strategy realizes with play against other strategies, S_{ij}, weighted by the other strategy's population weight, W_{jt}. These weighted scores are added up across the strategies in the game:

$$S_{it} = \sum_{j=1}^{N} W_{jt} S_{ij},$$

In effect, this gives the long-run average payoff that would have occurred had the generation of pairings been generated randomly with probabilities equal to the population shares divided by number of strategies. Players are never paired with themselves unless population shares are greater than one. The weight used for meetings between players using the same strategy is $W_{it} - 1$ for $W_{it} > 1$ and 0 otherwise. This sampling-without-replacement weighting scheme somewhat reduces the scores of nice strategies and improves the scores of nasty strategies; but it is the most reasonable method of representing interaction in small stable communities.

15. Had Tester followed an opponent's defection with only a single cooperative move, the relative performance of the Tolerant Moral program would have been improved compared to both Tit for Tat and Tester. Tit for Tat would get into a sequence of mutual defection with this version of Tester. By contrast, the Tolerant Moral program would obtain cooperation from such a Tester. A more recalcitrant version of Tester would worsen the score of Tolerant Morality.

16. By *opportunity cost of exit* we mean the sequence of payoffs that a player gives up by ending interaction with another particular player.

17. More generally, tolerance is advantageous if either behavior of occasional defectors can be changed by exit or defections are sufficiently rare that overall cumulated payoffs are positive. With the game payoffs used here, gains from interacting with a random strategy with a probability of more than .66 cooperations in games played would be to the benefit of tolerant strategies. Random strategies generally do not do very well in games with Tit for Tat players, however.

18. A change in the rank order of the payoffs can affect the results. In some settings, the exit option can generate a payoff smaller than the *mutual defect* result. For example, individuals might provide suboptimal levels of a public good that yields benefits, rather than costs, for fellow players. In this case, the exit option generates a smaller level of the public good, hence smaller payoffs. However, it is not clear whether what we have called a prudent moral strategy requires withdrawal from all such games. Exercise of the exit option (the one used in the simulations) requires exit only in cases where the previous round of play made one worse off than one would have been by not playing the game. Moreover, it bears noting that solutions to public goods problems often require commitments to joint production that leave cooperating players worse off if others do not adhere to their commitments.

19. R. D. Alexander's critical assessment of this tradition is summarized in the statement, "Assuming that morality has to be self-sacrificing is what has led moral philosophers into their worst intellectual quagmires" (1957, 151).

20. Gorden Tullock writes: "Where there are a number of potential players available, the dilemma is proportionally weakened. Indeed, here we have what amounts to a mapping of the usual economic distinction between a monopoly on the one side and competition on the other. . . . Where the market is broad and there are many alternatives, you had better cooperate. If you choose the noncooperative solution, you may find you have no one to noncooperate with" (1985, 1081).

21. It also explains why the incidence of feuds (vicious circles of mutual retaliation) seems to be higher for relations where exit is essentially ruled out.

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ETHNIC DEMOCRACY AND THE LEGAL CONSTRUCTION OF CITIZENSHIP: ARAB CITIZENS OF THE JEWISH STATE

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The citizenship status of its Arab citizens is the key to Israel's ability to function as an ethnic democracy, *that is, a political system combining democratic institutions with the dominance of one ethnic group. The confluence of republicanism and ethnonationalism with liberalism, as principles of legitimation, has resulted in two types of citizenship: republican for Jews and liberal for Arabs. Thus, Arab citizens enjoy civil and political rights but are barred from attending to the common good.*

The Arab citizenship status, while much more restricted than the Jewish, has both induced and enabled Arabs to conduct their political struggles within the framework of the law, in sharp contrast to the noncitizen Arabs of the occupied territories. It may thus serve as a model for other dominant ethnic groups seeking to maintain both their dominance and a democratic system of government.

The problem of maintaining viable democratic regimes in ethnically divided societies promises to be one of the more salient political issues of the new world order. Contrary to the expectations of modernization theorists in the 1950s and 1960s, "nation building" has not only failed in the Third World but is crumbling in the Second and is showing growing signs of strain even in the First. As Western liberal democracies are increasingly confronted with the reality of ethnic diversity and as multiethnic societies in Eastern Europe and elsewhere are struggling with the process of democratization, the efforts to reconcile competing ethnic claims with democratic values and institutions are bound to be more and more difficult to sustain.

One successful example of a democratic yet deeply divided society is Israel. A Jewish state by self-definition, Israel has a Palestinian Arab minority of about 15%, and its Jewish population is divided almost equally between Jews of European and Asian–African origin. Still, since its establishment in 1948, the country has maintained a stable democratic regime that has withstood five major wars, 12 national elections, massive immigration waves, and at least one serious economic depression. Moreover, the political stability within Israel itself has been highlighted, since December 1987, by the national uprising of the Palestinian Arabs in the territories occupied by Israel since 1967 (Lustick 1980; Smooha 1978, 1990).

An important reason behind Israel's ability to manage ethnic conflicts within the framework of the law is the particular character of its political system. This has recently been described as an *ethnic democracy* as distinguished from a majoritarian, consociational, or *Herrenvolk* democracy. An ethnic democracy is a polity combining "the extension of political and civil rights to individuals and certain collective rights to minorities with institutionalized dominance over the state by one of the ethnic groups" (Smooha 1990, 391).

Israeli ethnic democracy has been constituted, according to Smooha, by the tension between two political commitments: one to the Jewish character of the state and the other to a democratic form of government (Smooha 1990, 391–95). Other social scientists have discussed this tension in terms of particularism versus universalism, collectivism versus liberalism, primordialism versus civic culture. Whatever the designation, the current scholarly consensus is that the ethnic Jewish element in Israel's political culture has been on the ascendance since 1967 and that the universal, democratic element has been on the retreat (E. Cohen 1983, 1989a; Kimmerling 1985; Medding 1990, 233; Weissbrod 1983).

I shall argue that the portrayal of Israel's political culture as constituted by these two conflicting principles cannot adequately explain one of the key features of Israeli democracy, namely, the citizenship status of its Arab citizens. In particular, I will argue, this dichotomous view would render a contradictory account of the way in which Arab citizenship has been codified and made explicit by a series of legislative and judicial acts during the 1980s.

To account for these developments, it would be more useful to understand Israeli political culture as comprised not of two constitutional principles—liberalism and ethno-nationalism—but of three—republicanism, liberalism and ethnicity (see Smith 1988). Of the three, the republican principle has been easily dominant; but it has increasingly been challenged in recent years in the name of the other two.

The confluence of these three principles of legitimation has resulted, I will show, in two types of citizenship: republican for Jews and liberal for Arabs. Thus, while Jews and Arabs formally enjoy equal citizenship rights, only Jews can exercise their citizenship as practice, by attending to the common good. Nevertheless, the citizenship rights enjoyed by Israeli Arabs are not inconsequential. Since they were first laid down in 1948, these rights have become the foundation of a civic status that both induces and enables Israel's Arab citizens to confine their political struggles within the boundaries of the law.

My aim here, however, is not only to analyze a set of constitutional arrangements that have enabled a stable democratic regime to be sustained in a deeply divided society. I also hope to contribute to the extension of the current discussion of citizenship into the sphere of ethnic relations. The need to incorporate the problematic of ethnicity into the analysis of citizenship has frequently been pointed out. Thus, Turner has argued that "any further development of the theory of citizenship will have to deal more fundamentally with . . . the conditions under which citizenship can be formed in societies which are, as it were, constituted by the problems of ethnic complexity" (Turner 1990, 212–13). So far, however, with the exception of the issues of immigration and civil rights—which have mainly to do with admission to, rather than the definition of, citizenship (Brubaker 1989; Gibney 1988; Shklar 1991)—the field of ethnic relations has remained largely untouched by the citizenship debate. This in sharp contrast to the important ways in which the conventional views of citizenship, both liberal and republican, have been challenged by, for example, feminist writers (Jones 1990).

THE CONCEPTUAL BACKGROUND: LIBERAL VERSUS REPUBLICAN CITIZENSHIP

In the *liberal* tradition citizenship has been understood as "a set of normative expectations specifying the relationship between the nation-state and its *individual* members which procedurally establish the rights and obligations of members and a set of practices by which these expectations are realized" (Waters 1989, 160; emphasis mine).

The essence of the liberal conception of citizenship lies in its individual, universal, and equal character. As formulated by Roche, a community of citizens must possess at least three characteristics:

> Firstly, . . . all [members] have a presumptively equal status, equal rights and duties, etc. Principles of inequality deriving from gender, ethnic, class or other contexts are not supposed to be of relevance to the status of citizenship as such. Secondly, citizens accept that in principle and in fact they are and will remain strangers to each other. . . . They are not complete strangers, however. . . . They have impersonal but fairly complex and occasionally modifiable sets of assumptions and expectations about each other. . . . Thirdly, the presumptive equality between these fellow strangers is assumed to normatively govern, and to be enforceable in, their interactions with each other. (1987, 376–77)

The abstraction of the self from all particularity as a condition of citizenship in liberal democratic theory has been criticized by the proponents of republicanism. This "unencumbered self," whose sole characteristics are its subjectivity and its possession of natural rights, is denied, according to Sandel (1984), the possibility of membership in a moral community; for a moral community presupposes a notion of the common good antecedent to the individual citizen's choice. This contradicts the most basic liberal tenet of the priority of right over good (pp. 85–91).

To be able to participate in a moral community, Sandel has argued, one needs "character"—which the abstract, a-historical, unencumbered, liberal self does not possess. Having character means knowing "that I move in a history I neither summon nor command, which carries consequences nonetheless for my choices and conduct. It draws me closer to some and more distant from others; it makes some aims more appropriate, other less so" (Sandel 1984, 90–91). A shared moral purpose, from which originate both a notion of civic virtue and criteria for membership in the community, is thus at the heart of the republican ideal of citizenship (Geise 1984, 27).

Unlike the liberal citizen, who is the passive bearer of *status* (a mere bundle of rights), the republican citizen experiences citizenship as *practice* (active participation in the determination, protection, and promotion of the common good). The will and capacity to participate constitute one's civic virtue and are not only an expression, but also a condition, of citizenship (Oldfield 1990, 1–11). This raises the question how the republican community is constituted and what qualities are required for active participation in it.

Historically, the question of membership in the republican community was unproblematic, since membership was determined largely by birth. In the current revival of republicanism in the United States, however, two distinct notions of community may be discerned: a *weak* community, constituted by deliberation, membership in which is essentially voluntary and a *strong*, historical community that is discovered, not formed by its members (Friedman 1989, 283–84; Sandel 1982, 150; Sunstein 1988, 1541–42, 1571; Taylor 1989, 170).

The civic virtue appropriate to the weak community is no more than the willingness to be public-spirited—to place the general interest ahead of one's own in deliberating the common good (Geise 1984, 27). In the strong community, however, the possession of civic virtue requires much more than that. Since it is a historically constituted entity, members of this community must be committed to its ongoing existence as an important value in and of itself. As Sandel has put it, "[To] family or community or nation or people . . . I owe more than justice requires *or even permits*, not by reason of agreements I have made but instead in virtue of those more or less enduring attachments and commitments which taken together partly define the person I am" (1982, 179; emphasis mine; see also Taylor 1989, 182).

While the weak community is inclusive and open (at least in principle), people who are formally members can still exclude themselves from full citizenship practice by shying away from active participation in politics (Oldfield 1990, 160–61). In the strong community the problem is much more severe; for this type of community is necessarily exclusive (as many critics have charged: I. Shapiro 1990, 198–99; Smith 1988; Sunstein 1988, 1539–40). Moreover, the basis for exclusion from the strong community is at least partly ascriptive, since membership in Sandel's "enduring attachments" cannot be entirely a matter of choice. Since republican communities could not exist as islands unto themselves even in antiquity—much less today—the question arises, What of members of society who cannot or would not become full members of the community?

While millions of people officially defined as guest workers, undocumented aliens, and so on, live as noncitizens on the margins of many democratic societies, a permanent status equivalent to the metics of the ancient polis is no longer acceptable to modern democratic theory (Dahl 1989, 119–31; Walzer 1983, 53–61). The only solution, therefore, for people who cannot acquire full republican citizenship is to grant them a residual, truncated status, similar to the liberal notion of citizenship as a bundle of rights. Bearers of this citizenship as status do not share in attending to the common good but are secure in their possession of what we consider essential human and civil rights (Oldfield 1990, 160–61). Precisely this type of citizenship is possessed by Israel's Arab citizens.

ISRAEL'S POLITICAL CULTURE

Israel's political culture and its constitutional arrangements are rooted in the experiences of the Yishuv, the Jewish community in Palestine under the British Mandate (1922–48). Jewish settlement in Palestine, which began in the 1880s, was conducted through most of its prestatehood period under the ideological banner of Labor Zionism. The leading ethos of that period was *chalutziyut* (pioneering)—the mutual redemption of the Jewish people and their land through physical labor, agricultural settlement, and military defense.

As a political community the Yishuv was semivoluntary in nature under the aegis of the British Mandate. *Chalutziyut* served as its civic virtue, the criterion by which (in theory at least) civic recognition was accorded to individuals and social groups. Thus, a distinction was made not only between Jews and Arabs but also between the (mostly European) Jews who came to Palestine to "build and be built" there and the (mostly non-European) Jews who were regarded simply as immigrants (E. Cohen 1983, 115–16; idem 1989a, 150; Eisenstadt 1948; idem 1950, 200–203; idem 1985, 102–24, 297–300; Horowitz and Lissak 1978, 120–56; Shafir 1989, 91–122; Y. Shapiro 1977; idem 1984).

As the "civil religion" of the Yishuv (Liebman and Don-Yehiya 1983, chap. 2), *chalutziyut* had a distinctly strong republican character. Like most nationalist ideologies, it embodied a collective sense of mission for an ethnically defined community and expressed it in a powerful founding myth. Israel's Declaration of Independence, which concluded the Yishuv period, proclaimed:

> The Land of Israel was the birthplace of the Jewish people. Here their spiritual, religious and national identity was formed. Here they achieved independence and created a culture of national and universal significance. Here they wrote and gave the Bible to the world. . . . Impelled by this historic association, Jews . . . in recent decades . . . returned in their masses. They reclaimed the wilderness, revived their language, built cities and villages, and established a vigorous and ever-growing community. (quoted in Medding 1990, 237; see also Y. Shapiro 1977, 10)

Two other strong republican elements in the Yishuv's political culture were a powerful connection between military and civic virtue and a collectivist economic philosophy that assigned great, if not overwhelming, significance to the achievement of national economic goals through the public sector. While individual rights and the procedural rules of democracy were widely respected, they were clearly seen, in line with republican thinking, as secondary in value to the collective Zionist mission. Thus, the liberal notion of the priority of right over good had a very weak grounding in Israeli political culture (Aharoni 1991; Y. Shapiro 1976, 2–4; idem 1977, 19, 29–30, 37, 41; Sternhel 1991; see also Kerber 1988, 1665; Sunstein 1988, 1551, 1564–65).[1]

The ethos designed to legitimate the transition to statehood in 1948 was termed *mamlachtiyut*.[2] It emphasized the shift from sectoral interests to the general interest, from semivoluntarism to binding obligation, from foreign rule to political sovereignty. Equal application of the law was of paramount importance if the state was to assert its authority over the various Jewish social sectors, which had enjoyed a large degree of autonomy in the Yishuv. These sectors (identified by their brand of Zionism—Labor, bourgeois, or religious) had maintained their own educational networks, health care systems, labor exchanges, and even military forces. They all had to be brought now under the jurisdiction of the state.

Israel's first premier, David Ben Gurion, was the great preacher of *mamlachtiyut* and its key element, the uniform rule of law:

> Only in a state in which everyone—citizen, soldier, official, minister, legislator, judge and policeman—is subject to the law and acts according to the law; only in a state in which there is no arbitrariness, neither of ministers or rulers, nor of representatives of the people, and also not of individuals and political leaders—only in a state such as this is freedom guaranteed to the individual and to the many, to the person and to the people. (quoted in Medding 1990, 138)

The state envisioned by Ben Gurion was not, however, a neutral,[3] nightwatchman, liberal state. It was to continue to be committed to the values of *chalutziyut* and to demand such commitment from its citizens:

> Even if in their private lives they act as *chalutzim*, both the individual, and the organizations of individuals, will fail if they do not put their *chalutzic* activity in the service

of the state, and if the state's financial, organizational and legislative power is not committed to the *chalutzic* tasks that are thrust upon us." (quoted in Medding 1990, 136)

Mamlachtiyut, then, was not meant to displace *chalutziyut* as the legitimating ethos (or "civil religion") of society but, rather, to subsume it within the organizational framework of the state. And individuals and social groups were to continue to be evaluated on the basis of their contributions to the common good as defined by the Zionist project (M. Cohen 1987, 201–59; Horowitz and Lissak 1978, 189–95; Kimmerling 1985, 272; Liebman and Don-Yehiya 1983, chap. 4; Medding 1990, 134–77).

The most important statutory manifestation of Israel's commitment to its Jewish moral purpose is the Law of Return. Enacted in 1950, this law guarantees immediate right of entry to every Jew who comes to Israel and expresses a wish to settle there (with the exception of those who might pose a danger to public health or security). The Nationality Law grants every person admitted under the Law of Return Israeli citizenship from the day of arrival in the country (Kraines 1976, 95–99; Kretzmer 1990, 18, 31n., 36–40).

When Ben Gurion submitted the Law of Return to the Knesset for its approval, he argued that the preferential treatment accorded to Jews in the proposed statute did not affect the equal citizenship status of Jews and non-Jews, because the rights granted to Jews—and only to Jews—in the Law of Return were not *given* to them by the state. These rights predated the state, and Jews had possessed them by virtue of being Jews. Moreover, the State of Israel itself came into being through the right Jews had always had to the Land of Israel and in order to enable them to fulfill that right. Therefore, Israel could neither grant nor deny Diaspora Jews the right to settle in the country. In its positive acts, Ben Gurion vouched, the state would always maintain strict equality between its Jewish and non-Jewish citizens (Israel, Knesset 1950, 2035–37).

Ben Gurion's argument that the state was committed to the fulfillment of a primordial Jewish right to the land—a right from which the state derived its very purpose—was a clear affirmation of republican, as opposed to liberal, principles. Liberals have recognized the right of "communal self-determination," that is, the right of a community to shape its own cultural character or provide refuge to its ethnic kith and kin (Walzer 1983, 35–51). Otherwise, few immigration laws would be legitimate from a liberal point of view. However, Ben Gurion's claim that the state was morally bound to promote some good that was antecedent to it and that, moreover, this justified making distinctions between the rights of different groups of citizens (or potential citizens) on the basis of ethnic affiliation is anathema to liberal political theory. It is perfectly acceptable, however, from a strong republican point of view (see Smith 1988).[4]

To sum up, then, the dominant strain in Israel's political culture may be termed *ethnorepublicanism*. Jewish ethnicity is a necessary condition for membership in the political community, while the contribution to the process of Jewish national redemption is a measure of one's civic virtue. This conception necessarily excludes the Arabs. As non-Jews, they cannot belong to the ethnically defined community; as those from whom the land is to be redeemed they cannot partake of Zionist civic virtue. I shall now show how, starting with this a priori exclusion, a liberal citizenship status has been constructed for Israel's Arab citizens.[5]

THE FOUNDATIONS OF ARAB CITIZENSHIP

At the conclusion of the 1947–49 Arab–Israeli war, 63,000 of the 160,000 Arabs who had remained in the territory of the State of Israel were granted immediate Israeli citizenship. The others could apply for citizenship under the Nationality Law promulgated in 1950 (Moshe Shapira in Israel, Knesset 1950, 2134–35).[6]

This granting of Israeli citizenship to the country's Arab residents constituted a renunciation of the ethnonational principle, which would have mandated the institution of a South Africa–style *Herrenvolk* democracy for Jews only. That the *Herrenvolk* democracy option was not even raised by any serious political force in the Yishuv attests to the weakness of ethnonationalism as a legitimating ideology among Jews in Palestine at the time. Similarly, ethnically blind liberal citizenship could not have become the norm, either; for this would have meant forfeiting the most essential goals of Zionism. Ethnorepublicanism, on the other hand, was not only a natural continuation of the tradition of the Yishuv but also the most attractive option available to Israel's founders; for ethnorepublican citizenship could reconcile the uniform rule of law with the assignment to Jews of "more than justice permits."

The extension of citizenship rights to Arabs did not stem, however, solely from Israel's commitment to equality before the law. Attenuating circumstances made that decision both easier and more necessary than it might have been. The Arab population of Israel had been reduced by the war from a 2:1 majority (in the area of Mandatory Palestine) to a small minority of about 12.5% of the population of the State of Israel (Lustick 1980, 48–49; Morris 1987; Kamen 1987–88). Moreover, the Jewish leadership believed "that the minority problem had been all but eliminated by the mass Arab exodus—that the number of Arabs left in the state was insignificant" (Lustick 1980, 53). This made it easier to include the remaining Arabs in the body politic of the state.

In addition, the international situation had to be considered, as well. In the deliberations leading to the partition of Palestine in 1947, the Zionist representatives had emphasized that "when we speak of a Jewish State we do not have in mind any racial state or any theocratic state, but one which will be based upon full equality and rights for all inhabitants without distinction of religion or race, and without domination or subjugation" (Robinson 1947, 208). The UN Partition Resolution itself, which had legitimated Israel's founding in international law, required that elections to the legislative organ of the state be conducted on the basis of "universal suffrage" and that the state guarantee "all persons equal and nondiscriminatory rights in civil, political, economic and religious matters and the enjoyment of human rights and fundamental freedoms, including freedom of religion, language, speech and publication, education, assembly and association" (quoted in Medding 1990, 11–12).

Arab delegations at the United Nations complained about the treatment of the Arabs in Israel, and Israel's first application for membership in that body was refused in December 1948 (Lustick 1980, 61). Furthermore, the government sought to avoid the possibility that the remaining Arabs, who were concentrated in clearly demarcated geographical areas, would demand secession from Israel on the basis of their right of national self-determination—as has, indeed, happened in the territories occupied in 1967, whose residents have not been granted Israeli citizenship (ibid., 61–63).

After some debate in Ben Gurion's inner circle, Arab citizens were allowed to participate in the first general elections held in January 1949—at a time when the war had barely ended and prior to Israel's second, and successful, application for membership in the United Nations. Ben Gurion's reason for allowing the Arabs to vote was instructive: "We must not begin with national discrimination" (quoted in Medding 1990, 26). Significantly, however, over 60% of the Arab voters supported Ben Gurion's ruling party, Mapai, or its Arab affiliates, in these first general elections. This electoral pattern was maintained until 1965 (Rouhana 1986, 136).

The gap between the Arabs' formal citizenship status and their actual treatment (in this early period) as occupied enemy population was bridged by emergency decrees and administrative regulations. Most conspicuous of these was the military administration instituted in the areas of Arab concentration, "which imposed severe restrictions on their freedom of movement and economic opportunities, and placed them under surveillance and military law" (Medding 1990, 25; see also Dowty 1988; Jiryis 1976, 15–55; Kretzmer 1990; Lustick 1980).

Several Jewish political parties opposed the military administration, either because it violated the Arabs' citizenship rights or because it was seen as an instrument in the hands of the ruling party to secure for itself the lion's share of the Arab vote. In response to this pressure, many of the restrictive regulations against the Arabs had been gradually relaxed, beginning in the early 1960s. The final abolition of the military administration came, however, only in December 1966, following Ben Gurion's resignation from the government and the development of shortages in the (Jewish) labor market (Jiryis 1976, 31–55; Shalev 1989, 108; Smooha 1989, 15, 207).

One important *obligation* still denied to most Israeli Arabs is military service, which is mandatory for most Jewish citizens. This exemption is not a matter of law but of administrative practice and is probably unavoidable as long as the primary task of the Israeli military is to fight Arabs. Still, many social rights in Israel have been tied to the performance of military service, so that most Arab citizens either do not enjoy them or are entitled to smaller benefits (Kretzmer 1990, 98–107).

Moreover, the possibility of instituting some form of alternative national service for Arabs (which has been done in the case of religious Jewish women) has not been seriously considered by either the state or the Arabs themselves. Since any notion of citizenship entails the equality of rights and obligations—and especially in view of the extraordinary importance accorded national service in the republican view of citizenship (Oldfield 1990, 166–67)—this omission is highly symbolic of the Arabs' exclusion from attending to the common good. It has also become a major issue in the debate over the rights of Arab citizens. Thus, in the general elections of 1988 three right-wing Jewish political parties demanded that national service for Arab citizens become a condition for their enjoyment of full citizenship rights (Horowitz and Lissak 1989, 39–40; Nakdimon et al. 1988, 75, 133, 142; Smooha 1989, 96, 224).[7]

THE CODIFICATION OF ARAB CITIZENSHIP

The Challenge of al-Ard

The first major constitutional challenge to the citizenship status of Israeli Arabs came in 1965, an election year. Israel's electoral system is a proportional system, in which candidate lists submitted by political parties compete in a single national constituency for a share of the Knesset's 120 seats. These candidate lists have to be ratified beforehand by the Central Elections Commission (CEC), a semijudicial body made up of representatives of all parties currently in the Knesset and presided over by a Supreme Court justice.

Until 1965, the only Arab parties that participated in national elections were those affiliated with Zionist Jewish parties (except for the Israeli Communist party, which had been taking on more and more the character of an Arab party). In 1965, however, one of the lists of candidates submitted to the commission was the Socialist list organized by al-Ard (the Land), a small group of nationalist Arab intellectuals that had been previously declared an unlawful association by the minister of defense (Jiryis 1976, 187–96). The commission decided to disqualify the Socialist list on the grounds that it was "an unlawful association, because its promoters deny the [territorial] integrity of the state of Israel and its very existence" (Kretzmer 1990, 24). The commission's decision was appealed to the Supreme Court, which had to face the full force of the contradiction between the different constitutional principles governing the citizenship status of Jewish and Arab citizens.[8]

By the legal situation that existed in 1965, the Central Elections Commission did not have the authority to turn down a list of candidates if that list had met all the technical criteria stipulated in the Elections Law (such as a certain number of signatories and candidates' ages, citizenship status, etc.). In other words, the commission had no statutory authority to pass judgment on the list's political goals or those of its members. Strict adherence to the existing law would have mandated the Court to uphold the appeal of the Socialist list—as was, indeed, recommended by one of the three justices. The other two justices, one of whom was the president of the Supreme Court, decided to deny the appeal on the grounds that the gravity of the issue placed before the commission justified its departure from the strict letter of the law (Gavison 1986; Kretzmer 1990, 22–26; *Yardor* v. *CEC* 1965; see also Smith 1988, 243–44).

There was some unclarity in the Court's written decision, however, as to what precisely was the issue before the commission: Was it that the al-Ard group objected to the Jewish character of the state or to its very existence? But if the main opinion for the majority, written by President Agranat, is read in its entirety, it becomes quite clear that the two issues were one and the same in his mind (*Yardor* v. *CEC* 1965, 384–89; see also Gavison 1986, 179, 181). Thus, Gavison has interpreted Agranat's opinion to mean that

> Israel's being a Jewish state established as the fulfillment of the Jewish people's dream of self-determination justifies, as a necessary condition, the disqualification of a list which does not accept the Jewish people's right to a state of its own (as distinguished from a list which accepts this right but objects to some of its political and legal manifestations or questions the conditions under which a Jewish state should continue to be maintained in Israel). (1986, 159)

Evidently, the Socialist list was judged to deny the Jewish people's right to a state of its own.[9]

Between 1965 and 1984 no Arab party had attempted to field a list of candidates in Knesset elections. (In 1980 a public meeting called by Arab organizations to discuss the possibility of forming an all-Arab political party was banned by the government.) Instead, Arab voters had been shifting their votes from Mapai and its successor, the Labor party, to the Israeli Communist party, whose following has become overwhelmingly Arab. Thus, the communists gained 51% of the Arab vote in 1977 and have been receiving about a third of the Arab vote since then (Rouhana 1986).

During the 1970s, following the abolition of the military administration in 1966 and the Six Day War of 1967, Israeli Arabs were becoming increasingly assertive of their rights and had shown growing willingness to use the political system for promoting those rights. A number of countrywide representative organizations had emerged in that period, such as the Committee of Heads of Arab Local Councils and the Committee to Defend the Lands (established to fight the expropriation of Arab-owned land by the state). These organizations have gained the grudging de facto, if not de jure, recognition of state authorities (al-Haj and Rosenfeld 1989, 1990; Lustick 1980, 246–49; Smooha 1989, 211–12).

1984

The ethnorepublican principle governing the status of Israel's Arab citizens was next seriously challenged before the general elections of 1984. This time ethnorepublicanism was contested by two political parties: a joint Arab–Jewish party, the Progressive List for Peace (PLP), headed by a former member of al-Ard and Rabbi Meir Kahane's ultranationalist Jewish party, Kach. The two parties couched their arguments in the language of liberalism and Jewish ethnonationalism, respectively (on the PLP see Lustick 1990; Rouhana 1986; Smooha 1990; on Kach see Peled 1990a, 1990b; Shafir and Peled 1986; Sprinzak 1986).

The joint premise shared by the PLP and Kach was that because Israel had a sizable Arab minority, the claim that it could be both Jewish and democratic at the same time was a charade. A state defined as Jewish could not treat its non-Jewish citizens as equals. A democratic state (by any acceptable sense of the term) could not give preference to any particular group. While agreeing on the need to choose between these two constitutional principles, the two parties differed diametrically in the choices they recommended. The PLP advanced the liberal argument that all discriminatory laws and practices favoring Jews should cease to exist and that the territories occupied by Israel since 1967 should be turned over to their Palestinian inhabitants. Kahane's party argued, in the name of Jewish ethnonationalism, that all Arabs, citizens and noncitizens alike, must be expelled from Israel and the occupied territories, and that the territories should be incorporated into Israel.

The Central Elections Commission decided to disqualify the PLP list because "this list believes in principles that endanger the [territorial] integrity and existence of the State of Israel, and [the] preservation of its distinctiveness as a Jewish state in accordance with the foundations of the state as expressed in the Declaration of Independence and the Law of Return" (Kretzmer 1990, 27; *Neiman* v. *Chairman of the CEC* 1985, 238). The Kach list was disqualified, as well, because the party was found to

> uphold racist and anti-democratic principles which contradict the Declaration of Independence of the State of Israel, openly supports acts of terror, tries to incite hatred and hostility between different sectors of the population in Israel, aims at offending the religious feelings and values of some of the citizens of the state and negates in its goals the foundations of democratic government in Israel (*Neiman* v. *Chairman of the CEC* 1985, 238).

Both parties appealed the commission's decisions to the Supreme Court and were reinstated into the race. The Court ruled, on the basis of the 1965 al-Ard precedent, that in the absence of specific legislation empowering it to do so, the commission could not disqualify the Kach list for the racist and anti-democratic nature of its political platform. In the case of the PLP, the court decided that the commission acted within its authority in disqualifying the list for seeking to negate the existence of the state but that sufficient evidence had not been presented to the commission to merit such a determination. A number of the justices called upon the Knesset to enact legislation that would broaden the authority of the commission to disqualify candidate lists (Gavison 1986, 184–95; Kretzmer 1990, 27; *Neiman* v. *Chairman of the CEC* 1985, 226–33).

While in the case of Kach the implication of the Court's decision was quite clear, namely, that there was no legal way to bar from the electoral process a party seeking to expel the Arab citizens from the country, the implications of the PLP decision depended on an interpretation of the 1965 al-Ard case. If the Court is understood, in the earlier case, to have equated negating the Jewish nature of the state with a negation of its very existence, then one would have to conclude that the 1984 PLP decision meant that "a list cannot be permitted to stand for election if it denies the Jewish character of the state" (Klein 1985, 414; see also Gavison 1986, 159, 179, 181).

If a more restrictive interpretation of the 1965 decision is adopted, however—as was evidently done by the Court in 1984 (Justice Moshe Beiski and Justice Menachem Elon in *Ben-Shalom* v. *CEC* 1988, 242, 257; Gavison 1986, 180–81, 184–85), then the existence of the state and the preservation of its Jewish character must be seen as two separate issues. In that case the al-Ard precedent would apply only to candidate lists that seek to negate the very existence of the state. As Kretzmer has put it, "What of a list that explicitly wishes to repeal the Law of Return, but is sincerely committed to achieving this by the legislative process alone? The statement of the judges in the *Neiman I* [v. *Chairman of the CEC* 1985] case would seem to imply that such a list, which is not committed to physical destruction of the State, may not be disqualified under the *Yardor* [al-Ard] precedent" (1990, 27). This question, which bears directly on the ability of Israel's Arab citizens to have their own vision of the common good represented in the political process, was to be substantially clarified (and, in the case of Kach, completely transformed) by a series of actions that followed the 1984 elections.

1988

Both Kach and the PLP succeeded in electing representatives to the Knesset in 1984, with Kach winning one seat (for Rabbi Kahane himself) and the PLP two. This outcome—especially the election of Kahane, a vociferous racist demagogue—prompted the government to introduce legislation that would bar such parties from contesting future elections. The statute, an amendment to Basic Law—the Knesset adopted on July 31, 1985, reads:

> A list of candidates shall not participate in elections to the Knesset if its goals, explicitly or implicitly, or its actions include one of the following:
> (1) Negation of the existence of the State of Israel as the state of the Jewish people;
> (2) Negation of the democratic character of the State;
> (3) Incitement of racism. (Israel, Knesset 1985, 3951).

It should be noted that this amendment states the particularistic norm of Israel's being the state of the Jewish people explicitly, while the universal norm of equality before the law appears only implicitly, under the more general term *democratic*. During the debate, two changes proposed by members of Knesset belonging to the Israeli Communist party and the PLP (both predominantly Arab parties) were voted down. Their proposals would have removed or mitigated the particularistic content of article 1 by having it close with either "the State of Israel" or "the State of Israel as the state of the Jewish people and its Arab citizens" (Gavison 1986, 198; Kretzmer 1990, 29).

In 1988, prior to the next Knesset elections, the Central Elections Commission was presented with petitions to disqualify, on the basis of the amended Basic Law, the candidate lists of both Kach and the PLP, as well as almost every other party planning to participate in the elections. Kach, for example, requested that all other parties then represented in the Knesset be disqualified, since they all violated either article 1 or article 2 of the amendment. This, of course, was in line with its position that the Jewishness of Israel and its democratic system of government were mutually exclusive.

The commission deliberated seriously the cases of only three parties: the PLP, Kach and Moledet (Homeland), a new, extreme, right-wing party, which, like Kach called for the "transfer" of Arabs from the Israeli "control system" (Kimmerling 1989) but, unlike Kach, limited its demand to the noncitizen Arabs of the occupied territories (Nakdimon et al. 1988, 142). The commission approved the request to disqualify Kach, rejected the request to disqualify Moledet, and was tied (along strict party lines) in its vote on the PLP. The tie was broken by the commission chair, a Supreme Court justice, who decided that the PLP would be allowed to run (Israel, CEC 1988a–d).

Two of the commission's decisions (those relating to Kach and the PLP) were appealed to the Supreme Court. The Court rejected both appeals—Kach unanimously and the PLP by a 3:2 majority (*Ben-Shalom* v. *CEC* 1988; *Neiman* v. *Chairman of the CEC* 1988; Kretzmer 1990, 29). The appeals provided an opportunity for the constitutional implications of the amended Basic Law to be examined by the country's highest judicial authority. While the Court's decisions—against Kach and in favor of the PLP—seemed to uphold the liberal, as against the ethnonational, principle in the constitutional makeup of the state, a closer reading of the decisions reveals a much more complicated picture.

The fundamental constitutional, philosophical, and ideological issues involved in the decision to reject the appeal against the PLP were discussed thoroughly only in the opinions of the two justices who were in favor of upholding it. The three majority justices, including the president of the Supreme Court, rejected the appeal, like the Court did in 1984, for lack of sufficient evidence against the PLP. President Shamgar referred approvingly to the opinions of the two dissenting justices (*Ben-Shalom* v. *CEC* 1988, 246), who also concurred with each other (pp. 232, 257). The two other majority justices did not even refer to the opinions of their dissenting colleagues.

The opinions of the dissenting justices, buttressed by lengthy and profound analyses of the problem, were unequivocal. In the words of Deputy President Elon:

> The principle that the State of Israel is the state of the Jewish people is Israel's foundation and mission [*yessoda vi-yeuda*], and the principle of the equality of rights and obligations of all citizens of the State of Israel is of the State's essence and character [*mahuta ve-ofya*]. The latter principle comes only to add to the former, not to modify it; [there is nothing in] the principle of the equality of civil rights and obligations to modify the principle that the State of Israel is the state of the Jewish people, *and only the Jewish people*. (*Ben-Shalom* v. *CEC* 1988, 272; emphasis mine)

Moreover, according to Elon, the State of Israel was established for "the entire Jewish people, with all its individuals and with all of its communities, . . . both those living in the state and those residing in [the] diasporas and exiles" (ibid.). To claim that the State of Israel should be regarded as the state of its Jewish and Arab citizens, even if this claim is not accompanied by an explicit demand to abrogate the Law of Return,[10] is to call for turning Israel from a Jewish to a Jewish-Arab state. This is tantamount to negating the character of Israel as the state of the Jewish people. Since the PLP demanded complete equality between Jews and Arabs in Israel and extended this demand to the national character of the state, as well, it should have been excluded from the elections. The same argument was developed, at great length, by the other dissenting justice, Dov Levin (ibid., 234–46, 275–76; see also Kretzmer 1990, 30).[11]

Implications

Taken as a whole, the Supreme Court's decision in the 1988 PLP case meant that participation in Knesset elections could be legally denied to a list of candidates "that reject[ed] the particularistic definition of Israel as the state of the Jewish people, even if the list [was] committed to achieving a change in this constitutional fundamental through the parliamentary process alone." Moreover, in Kretzmer's view, the decision also implied that

> on the decidedly fundamental level of identification and belonging there cannot be total equality between Arab and Jew in Israel. The state is the state of the Jews, both those presently resident in the country as well as those resident abroad. Even if the Arabs have equal rights on all other levels the implication is abundantly clear: Israel is not *their* state." (Kretzmer 1990, 31; emphasis original)

This view is shared by Smooha as well: "From the Israeli–Arabs' viewpoint, the provision that Israel is the land of Jews all over the world, but not necessarily of its citizens, degrades them to a status of invisible outsiders, as if Israel were not their own state" (Smooha 1990, 402).

In the opinion of these two Israeli scholars, then, the 1988 PLP decision explicitly excluded Israel's Arab citizens from full membership in the community. Thus, the Court seemed to uphold the ethnonational principle of legitimation over the liberal–democratic one. However, to appreciate the full import of the PLP decision, it must be seen in the context of two other acts connected with the 1988 elections: the exclusion of Kach and inclusion of Moledet.

In the deliberations of the Central Elections Commission on the issue of Moledet, it was quite clear that at least in the minds of the commission members, there was a world of difference between advocating the "transfer" of both citizens and noncitizens (Kach) and calling for the expulsion of noncitizens only (Moledet).[12] Much of the debate in the commission turned on the question of whether or not Moledet was really interested in expelling noncitizen Arabs only or whether this limitation was a mere tactical ruse designed to evade the racism and antidemocracy clauses of the law (Israel, CEC 1988d). It was obvious to Moledet's opponents on the commission that they had to prove that its "transfer" plans included citizen, as well as noncitizen, Arabs. Since they failed to prove this to the satisfaction of their colleagues, the party was not disqualified. Nor was this decision even appealed to the Supreme Court, as the case of the PLP had been.

The decision to exclude Kach but not Moledet thus reaffirmed the distinction between citizen and noncitizen Arabs. It buttressed, in other words, the citizenship rights of Israel's Arab citizens. If viewed through the lens of ethnicity versus democracy, therefore, the decisions in the matters of the PLP, Kach, and Moledet together seem to point simultaneously in two different directions: they both limited and reinforced the Arabs' democratic rights.

I suggest, however, that the three decisions are perfectly consistent with one another. They constitute an affirmation of the ethnorepublican principle governing the Arabs' citizenship status over both the liberal and the ethnonational challenges mounted against it. Thus, a protective wall (so to speak) has been placed around Israel's Arab citizens—a wall that separates them from both the Jewish citizens, who can attend to the common good, and the noncitizen Arabs of the occupied territories, who are Israel's metics. Within the area confined by this wall, Arab citizens can securely enjoy (formally at least) the rights of liberal citizenship. They must not, however, challenge the existence of the wall itself.

CONCLUSION

Israel's Arab citizens are by no means satisfied with the place accorded them in Israel's political system (Smooha 1990). However, the limited, liberal citizenship status they enjoy has endowed them with sufficient rights and privileges to both enable and induce them to conduct their struggle within the constitutional framework of the state, rather than against it. This has been a key factor in allowing Israel to maintain a stable democratic regime in the context of an acute ethnic conflict.

The Intifada (Palestinian national uprising), which has been raging in the occupied territories since December 1987, has strained, but also underscored, the commitment of Israeli Arab citizens to abide by the law of the land. The Intifada greatly increased the momentum of Arab protest activity inside Israel (strikes, rallies, demonstrations, petitions to the government), which had been on the rise since the mid-1970s. Unlike the uprising in the occupied territories, however, these activities, with only marginal exceptions, have remained nonviolent and have been carried out within the framework of the law. The reason for this has been clearly articulated by Israeli Arab leaders. As Rouhana has put it: "the Arab leadership made clear that the Arabs in Israel would act only within the law. It was argued that their status is different from that of other Palestinians and therefore, that their efforts would be expressed differently" (1989, 47). Furthermore, the Israeli Arabs' increasing political confidence is fed, according to Rouhana, "by the sense of security that their status as Israeli citizens provides, as well as their formal equality before the Israeli law" (ibid., 54; see also E. Cohen 1989b, 85).

On a more fundamental level, their "Israeliness" is an important component of the Arabs' sense of collective identity, in spite of their growing self-identification as Palestinians. As Smooha has argued,

> The acquisition of various Palestinian identifications is manifested mostly in feelings of solidarity but not in a desire to dissociate oneself from Israel and to join any Palestinian entity. The overwhelming majority of the Arabs would be relieved by a settlement of the Palestinian problem, and in particular, 80% would feel better as Israeli citizens if a Palestinian state were to be established in the West Bank and Gaza Strip. Yet, only 8% of them . . . are willing to consider a move to such a state. (1989, 92–93)

Still, if my analysis is correct, there is a limit beyond which the Arabs' exercise of their citizenship rights will not be able to proceed. This limit is located at the transition point between struggling to have their liberal rights respected in the conduct of official policy and attempting to challenge the prevailing notion of the common good.[13]

As dominant ethnic groups in existing and aspiring democracies try to maintain both their dominance and a democratic system of government, they will very likely experiment with different varieties of two-tiered democracy. Thus, the constitutional arrangements that have made Israel into an ethnic democracy (especially the combination of republican and liberal citizenship) may become more widespread in the future (see Ginsburgs 1990).

The overt use of ethnicity (as of other particularistic markers, e.g., gender, class, or religion) to distinguish between different categories of citizenship is offensive to anyone committed to liberal values. It may serve us well, however, to remember that discrimination on the basis of ethnicity, race, religion, gender, and sexual preferences has not been unknown in liberal democracies. Moreover the very definition of citizenship in liberal political theory has had a particular bias built into it (as feminist writers have repeatedly pointed out):

> A citizen is one whose membership is contingent upon the ultimate subordination of the specific bonds of gender, race, and class—indeed, all particularized identities—in favor, most often, of a national identity and loyalty to the state. To become a citizen is to trade one's particular identity for an abstract public self. But the identification of citizens and the definition of citizenship is derived from the representation of the behavior of a group with particular race, gender, and class characteristics (white, male elites) as the model of citizenship for all individuals. (Jones 1990, 784; see also Hirsch 1986; Turner 1986, 46–47)

Thus, in the real world of democracy, the choice very often may not be between the faithful implementation of "All persons are created equal" and blatant group discrimination. It may be a choice between de facto discrimination under the guise of liberal constitutional principles and something resembling two-tiered democracy. Given this choice, an ethnorepublican arrangement whereby a subordinate ethnic group is confined to, but also made secure in the possession of, liberal citizenship rights and is allowed the political space to struggle for the consolidation of these rights may not be the worst possible outcome.

Notes

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1. It has been suggested that proportional representation is another characteristic of a republican polity (Sunstein 1988, 1585–89). Israel's electoral system is, of course, proportional in the extreme. It would be interesting to explore the connection between republicanism and proportional representation, both theoretically and empirically.

2. The conventional translation of *mamlachtiyut*, "statism," is unfortunate because of its protofascist connotations. Although *mamlacha* literally means "kingdom," *mamlachtiyut* conveys a sense of a common purpose, as determined by an all-encompassing social–political community and expressed

in the rule of law. Thus, the concept borders on Rousseau's *general will*.

3. Medding describes the Israeli state structures as "neutral," in the sense of being free of "partisan criteria" (1990, 9, 11). He also emphasizes that he does not mean to suggest that they are neutral in the class struggle (p. 28, n. 3). However, it would be exceedingly difficult to sustain the argument that the Israeli state is neutral as between Jews and Arabs (Smooha 1990, 393) or in the liberal sense of lacking a moral purpose.

4. Israeli jurists have tended to use liberal arguments in defense of the Law of Return. They have argued, for example, that as a law of immigration and naturalization and as a law that does not discriminate *against* any particular national or ethnic group, it does not violate the norms of international law (Smooha 1978, 399, n. 24). Ben Gurion, however, explicitly stated that the Law of Return was *not* an immigration law (Israel, Knesset 1950, 2037). Ironically, the fact that a similar law exists in the Federal Republic of Germany has also been adduced in support of the Law of Return (Kretzmer 1990, 45, n. 6). The Arab view of the Law of Return is that it gives the right to "return" to Jews who have never set foot in the country, while hundreds of thousands of Palestinian refugees are denied that right (see Smooha 1978, 197; idem 1990, 396). Another important, though largely symbolic, manifestation of Israel's moral purpose is the Nazi and Nazi Collaborators (Punishment) Law of 1950. In this law, Israel undertook to punish Nazi criminals for crimes committed before the state existed, outside of its territory, and in which neither the victims nor the perpetrators were its own citizens. The most celebrated case adjudicated under this law was that of Adolph Eichmann (Papadatos 1964, 32).

5. This sketchy description of Israeli political culture does not take account of much of its complexity or of changes that have occurred in it over time. The most comprehensive—though still very partial—account is Liebman and Don-Yehiya 1983.

6. The connection between the Law of Return and the Nationality Law had the effect, initially, of denying Israeli citizenship to many of the hundred thousand Arab residents not covered by the original grant of citizenship. This problem has been rectified, however, by a 1980 amendment to the Nationality Law (Kretzmer 1990, 37–39).

7. I do not provide a complete account of the status of Israel's Arab citizens, only review some of their civil and political rights. Of the authors cited, Smooha and Lustick provide the most comprehensive accounts of the situation of Israeli Arabs, while Kretzmer focuses specifically on their legal status.

8. In the absence of a formal written constitution and a bill of rights, the courts (especially the Supreme Court) have become the foremost guardians of civil rights and liberties in Israel (Arian 1989, 200–201; Lahav 1990). The Supreme Court has been playing that role primarily in its capacity as the High Court of Justice, entrusted by law with the authority to "issue orders against any body which performs a public duty under law" (Kretzmer 1988, 122). Although judicial review of legislation does not exist in the Israeli system, in recent years the Court has adopted an increasingly activist attitude vis-à-vis the Knesset, agreeing to pass judgment on all types of Knesset decisions, except primary legislation (Kretzmer 1988). The main reason for this development has been the inability of the other branches of government to make politically difficult decisions and their growing disregard for civil rights (Rubinstein 1991, 28). In what follows, I shall rely heavily on Supreme Court decisions in election appeals as indicative of changes in Israel's political culture.

9. It should be noted that the threat posed by the Socialist list was purely ideological. In the words of Justice Cohn, "In the material which was in front of the Central Elections Commission, and which was presented to us too, there was nothing to justify, let alone mandate, the finding that there is a [real or clear or present] danger" posed to the state or to any of its institutions by the activities of the list. This factual statement was not disputed by either of the other justices (*Yardor* v. *CEC* 1965, 381; see also Gavison 1986, 154). Gavison herself also believes that "principled commitment to the idea of Israel as the state of the Jewish people *does not* follow from, and is not mandated by, any conception of democracy. Therefore, if making such a commitment is allowed [under the law], then it may be justified to place certain limits on formal democracy in order to protect" that commitment (Gavison 1986, 160; emphasis original). This, in Sandel's (1984, 85–91) terms, is a perfect example of republican placing of good before right.

10. The PLP does not demand that the Law of Return be abrogated but, rather, that the right to return be extended to the Palestinian refugees, as well (D. Levin in *Ben-Shalom* v. *CEC* 1988, 241; Nakdimon et al. 1988, 118).

11. Justice Dov Levin objected specifically to, among other things, the preamble to the PLP platform, which is formulated in classic liberal terms: "In all of its struggles the List is guided by a paramount principle: Man is everything and the state is a tool in his hands. In this spirit the List shall struggle for another Israel—a state of all of its citizens" (*Ben-Shalom* v. *CEC* 1988, 240).

12. The distinction between expelling citizen and noncitizen Arabs is very much in the mind of the general Israeli public, as well. In an attitude survey conducted by Asher Arian in March of 1991, 39% of the respondents supported the idea of "transfer" when its intended victims were not specified; 34% supported it for noncitizen Arabs, and only 22% supported it for citizen Arabs (personal communication; see Peled 1990b, 356–57).

13. It has recently been suggested that the growing political polarization among Jews in Israel over the future of the occupied territories, coupled with the Arabs' rising numerical importance in the electorate, could result in Arab parties being included in the governing coalition in the not-too-distant future (Lustick 1989, 1990). But even disregarding the current massive immigration of Jews from the former Soviet Union, which has already diminished the Arabs' electoral weight (and which could not have been foreseen by Lustick), Arabs are not very likely to be allowed into the inner sanctum of political power in Israel in the foreseeable future. Even if the problem of national security could be overcome by having Arab parties support the government from "outside" (i.e., without sharing ministerial responsibility), the notion of a government dependent on Arab support runs against the grain of Israeli ethnorepublicanism (see Benvenisti 1991; Rabinowitz 1991).

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THE WELFARE STATE, TURNOUT, AND THE ECONOMY: A COMPARATIVE ANALYSIS

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While the economic voting literature is voluminous, comparatively little attention has been paid to the question of how—or whether—the economy affects turnout. I address this issue by examining national elections in 29 countries. Using time series data, the initial findings are replicated by a case study of American presidential and midterm elections since 1896. It is argued that the effect of economic adversity depends upon the degree of welfare state development. This relationship is argued to be nonlinear, so that mobilization occurs at either extreme while withdrawal obtains in the middle range. The importance to democratic theory, the study of elections, and the politics of welfare policy are discussed.

The past two decades have seen a tremendous amount of scholarly attention paid to the question of whether—and in what ways—macroeconomic conditions affect electoral outcomes. (For reviews, see Eulau and Lewis-Beck 1985; Monroe 1984). While this research program has become a veritable cottage industry, comparatively little attention has been paid to the related issue of how the economy affects turnout. Although much is known about how economic considerations affect the decision of how to vote, it remains to be seen how such considerations structure the decision of whether to vote.

The issue is relevant to a variety of theoretical concerns. If the economy does affect voter participation, our understanding of turnout is incomplete without a specification of this relationship. Similarly, without knowing how the economy affects who votes, it may be impossible properly to specify how the economy influences election results (Achen 1980). If turnout varies with the economy, there may also be implications for democratic responsiveness. If recession depresses turnout by keeping potential "punishers" from the polls, then government may escape retribution for economic mismanagement because the very people with the most interest in seeking such retribution may not vote (Rosenstone 1982).

TAKING STOCK

The basic proposition is that turnout deviates from an expected, or "normal," value in response to changes in the state of the macroeconomy. This relationship may take any of three possible forms. The first is what Schlozman and Verba (1979) call "mobilization." In this view, economic hardships encourage people to enter the political world as a means of seeking redress for grievances. If the literature on the "negativity bias" in political attitudes and behavior (see Lau 1982, 1984) can be accepted at face value, then individuals suffering hardship may be more strongly motivated than others to go to the polls to vent frustration at government's failure to manage the economy successfully.

The second possibility is that a sour economy encourages citizens to withdraw from the political process. Wolfinger and Rosenstone (1980) report, for example, that the poor and unemployed voted less in the 1972 presidential election (see also Caldeira, Patterson, and Markko 1985). Similarly, Rosenstone (1982) argues that economic adversity reduces turnout at both the individual and aggregate levels. Simply, economic downturns may foster preoccupation with personal problems that reduce the time and attention that may be paid to the more peripheral concerns of politics (Rosenstone 1982; Sniderman and Brody 1977).

The third possibility is the null of no effect. In this view, declines in financial well-being neither politicize nor alienate individuals. There is, again, some evidence supporting this hypothesis. Using survey data, Fiorina (1978) finds "no discernible relationship between economic conditions and voting turnout." (For similar conclusions, see Conway 1981.) Using aggregate data, Arcelus and Meltzer (1975) find insignificant or negligible relationships between congressional election turnout and several economic indicators, while Lane (1959) reports similar results for presidential elections. Schlozman and Verba (1979) also find that unemployment has essentially no effect on political consciousness, participation, or policy preferences.

The most striking aspect of the literature may be its inconsistency. In surveying it, Rosenstone (1982) concludes that mobilization, withdrawal, and no effect are all "plausible claims, but the conflicting and weak empirical findings make it difficult to choose one over the others." Although his own careful analysis makes a strong case for withdrawal, later findings do not uniformly support that conclusion (Southwell 1988).

In part, the confusion may result from measurement and methodological shortcomings.[1] Conceptual problems may also play a role. Longitudinal studies do not consider that response to economic adversity may vary temporally as social and political circumstances change. While cross-sectional studies avoid this problem by holding such factors constant, they are insensitive to the possibility that the results may not obtain outside of the particular economic or social setting. The lack of consensus in the literature may be a function of the fact that the phenomenon varies over time. Similarly, the exclusive focus on U.S. elections introduces a spatial constraint, as well.[2] It may be that the electoral importance of the economy varies both spatially and temporally due to contextual conditions that vary from place to place and time to time. One obvious possibility of such an intervening contextual variable is the extensiveness of security programs designed to insulate citizens against the arbitrariness of macroeconomic cycles.

DATA AND METHOD

The relationship between economic conditions and turnout is estimated with a standard linear model where the dependent variable is turnout, as measured by the proportion of the eligible electorate voting. The principal independent variable is the election year proportion change in real per capita national income.

Several controls are also introduced to account for noneconomic impacts on voter participation. One such term is a dummy variable representing sudden expansions of the electorate: the enfranchisement of large numbers of inexperienced voters may depress overall turnout. Another is an autoregressive measure of previous turnout. This variable represents any trend that is unrelated to economic performance. As such, it should capture all remaining noneconomic factors that systematically affect the vote series (Rosenstone 1982; see also Kramer 1971).

The data cover national elections for 29 countries from 1960 to 1987. Included are all countries with democratic electoral systems and a population over one million for which data are available.[3] Given that each case is an election in country *c* at time *t*, the resulting data set is a pool created across both spatial and temporal dimensions. As a consequence, the estimation procedure must account for the pooled structure of the data. (For a discussion, see Stimson 1985.) Because the source of potential bias is large between unit differences in the dependent variable (i.e., extreme international variation in the general level of turnout), the solution is to remove it by introducing a dummy variable for each spatial unit (except a reference category). In essence, this strategy estimates a "fixed effect" for each nation.

The inclusion of the dummies also serves a substantive purpose in that these terms, coupled with the lagged turnout variable, effectively embody the institutional, constitutional, and developmental factors that affect long-term turnout levels (see Jackman 1987; Lijphart 1984; Powell 1980, 1986). Thus, national differences in the overall pattern of voter participation are absorbed by the fixed effects. The moving average of past turnout (for each nation) should serve a similar function by providing a benchmark against which contemporary turnout can be judged, as well as adequately controlling for intrastate secular trends.[4]

A second issue that must be considered is the possibility that the relationship between the economy and participation may differ between wildly different political and socioeconomic environments. In part, of course, it is this consideration that necessitates the inclusion of variables that control for noneconomic factors. However, the distinction between the industrialized and developing worlds (or, equivalently, between nations with and without extensive social security provisions) may not be adequately tapped by such efforts. Because the model already includes a dummy for each country, it is impractical to include a developing world dummy and interaction term. Hence, to test for different economic effects in the different settings, I have simply divided the sample and performed the analysis separately for each set of nations:[5]

$$\begin{aligned}\text{TURNOUT} = a &+ b1(\text{INCOME}) + b2(\text{EXPECT}) \\ &+ b3(\text{NEWVOTERS}) + b4(\text{State}_1) \\ &+ b5(\text{State}_2) + \ldots + b31(\text{State}_{28}) + e,\end{aligned}$$

where TURNOUT is the proportion of eligible voters casting ballots at time *t*;[6] INCOME is the election year proportion change in real per capita national income;[7] EXPECT is expected turnout based upon past voting trends, calculated as a moving average of the turnout at times $t - 1$ and $t - 2$; NEWVOTERS is a dummy variable coded one for years in which there occurred sudden expansions of the electorate due to the enfranchisement of previously excluded groups, and zero otherwise;[8] State_n are dummy variables for each of the $n - 1$ countries included in the study; and *a* and *e* are the intercept and error terms, respectively.

RESULTS

As indicated in Table 1, the coefficients of INCOME are statistically significant for both the industrialized and developing world. Interestingly, however, both the magnitude and sign differ between the nation groups. In the former, the coefficient is .17, while in the latter it is −.75.

The substantive conclusion is clear: the economic effect in the first world is withdrawal, while in the developing world it is mobilization. In the former, real income is directly related to turnout, so that sour conditions depress turnout. In the latter, income is inversely related to turnout such that when things are bad, citizens tend to vote in greater numbers. Furthermore, turnout in the first world is less elastic in relation to economic conditions than in the third world. A 5% decline in national income lowers turnout by approximately 1% in industrialized nations, while in the developing world the same economic fluctuation increases turnout by more than 3.5%.

TABLE 1

Turnout and the Economy

REGRESSOR	INDUSTRIALIZED NATIONS	DEVELOPING NATIONS
INCOME	.170* (.081)	−.747* (.356)
EXPECT	.477** (.093)	.470* (.214)
NEWVOTERS	−.019 (.079)	.041 (.089)
CONSTANT	.407** (.080)	.280* (.127)
R^2	.918	.423
N	174	38

Note: Table omits country dummy variables. Dependent variable is turnout; entries are unstandardized regression coefficients (standard error).
*$P \leq .05$.
**$P \leq .001$.

The implication of the magnitude difference is that outside of the first world, citizens are more sensitive to economic conditions. In many ways this is unsurprising. The industrial countries have established reasonably generous security programs that mitigate against the most arduous affects of economic dislocation. In a situation where the level of economic attainment has reached the point where the sociopolitical system has significant cushions against minor economic perturbations, and where, as a result of that fact, many citizens may tend to be more politically sensitive to other than strictly economic issues, it may be that the economy has less of an impact on the decision to vote. Further, the emphasis on noneconomic issues may marginalize the immediate political importance of ordinary fluctuations in the business cycle. The effect, again, is to dampen the potential for either mobilization or withdrawal.

The more interesting (and unexpected) result is the qualitative difference in citizen reaction to economic performance. In the industrial world, poor economic performance tends to depress turnout in much the way that Rosenstone (1982) suggests. When faced with financial difficulties, citizens withdraw from the political process to devote attention to personal concerns. In the developing world, exactly the opposite occurs. Poor economic performance drives more voters to the polls.

The difference may be attributable to the stakes involved. In the absence of security programs, the potential human costs of poor economic performance are much greater. Thus, individuals may be considerably more sensitive to changes in the macroeconomy. In terms of costs and benefits, citizens may view the benefits of voting to be comparatively large when the economy is in decline. While the opportunity costs may in fact increase when the individual is faced with financial difficulties, the perceived benefits of voting may also increase in environments where citizens are more sensitive to short-term variations in economic performance. In industrialized nations, the benefits from going to the polls may be comparatively static. Hence, as a weak economy increases opportunity costs for many people, turnout can be expected to fall.

A commonly used measure of comparative spending on security programs is the percentage of gross domestic product devoted to government (or otherwise legislatively mandated) programs guaranteeing minimum income, health care, unemployment insurance, and pensions. (For a complete description, see International Labor Organization 1985, 1–10.) There is an enormous gulf between the developing and industrialized worlds on such spending. Using 1975 spending levels (roughly the midpoint of the time frame), no developing state spent more than about 5% of gross domestic product on social security, while many developed nations spent more than 20%. Even comparatively poor Western countries such as Portugal or Spain spent more than three times that of Turkey or Jamaica and nearly five times as much as India or Sri Lanka.

If the real issue behind the variable economic effect is the presence or absence of security programs, then presumably such would be apparent if this social security measure (SECURITY) and its interaction with INCOME were included in the model and the developing-versus-industrialized distinction dropped. Table 2 provides the results. The coefficient of INCOME is negative, while the interaction term is positive. The total impact of the economy on turnout at some level of security spending is the sum of (1) the INCOME coefficient and (2) the SECURITY × INCOME coefficient times the given level of spending.[9] Thus, the initial effect (at zero level of spending) is extreme mobilization, which is quickly dampened—and ultimately reversed—as a social commitment to security programs emerges. The absence of security provisions leaves economic discontent to be politicized. The introduction of such programs quickly reverses this effect. Withdrawal occurs as a level of security is established such that the most severe hardships of recession are blunted. In such cases, the deleterious effects of economic downturns are sufficient to cause preoccupation with personal affairs to the exclusion of now seemingly remote political concerns but no longer so threatening as to encourage political participation.

There is not, however, any a priori reason to believe that this pattern must necessarily be linear. Two logical alternatives are displayed graphically in Figure 1. The figure illustrates the economic affect on turnout as a function of welfare spending, that is, the total slope of INCOME (y-axis) at given levels of security spending (x-axis).

The first alternative is that the effect of security provisions may quickly level off after some minimum has been reached. Thus, the dampening of turnout in a sour economy might not be appreciably greater in Sweden or Denmark than it is the United States or

TABLE 2

Turnout and the Economy: Linear Security-Income Interaction Model (N = 212)

REGRESSOR	COEFFICIENTS
INCOME	−.580** (.207)
INCOME * SECURITY	.041* (.018)
SECURITY	.004 (.005)
EXPECT	.290* (.144)
NEWVOTERS	.020 (.025)
CONSTANT	.602** (.079)
R^2	.721

Note: P (INCOME < 0) > .997; P (INCOME * SECURITY > 0) > .989. Dependent variable is turnout; entries are unstandardized regression coefficients (standard error). Table omits country dummy variables.
*$P \leq .05$.
**$P \leq .01$.
***$P \leq .001$.

Australia. If true, this would imply a hyperbolic relationship between security spending and voter response to the economy, that is, $T = -aI/S + bI$, where T is the deviation of turnout from its expected value, I is INCOME, and S is SECURITY.

A second and equally plausible hypothesis is that a sufficiently large commitment to a security system further reduces the stress associated with economic dislocation and, with it, the opportunity costs so involved. Thus, one might expect withdrawal in the United States but no effect (or perhaps even mobilization) in Sweden. This would imply a second degree polynomial of the form $T = -aIS^2 + bIS - cI$, with terms as just defined.

The hyperbolic function requires that the interaction of the economic variable and the inverse of economic security spending be added to the original model. For the polynomial, the first-order economic and security interaction is retained and an additional interaction of INCOME and SECURITY^2 included. The results are reported in Table 3. In both cases, the coefficients are significant and of the correct signs. Further, the increase in the coefficient of determination (adjusted for the loss in degrees of freedom) in moving from the simple interaction (Table 2) to either

FIGURE 1.

The Economic Effect on Turnout as a Function of Welfare Spending

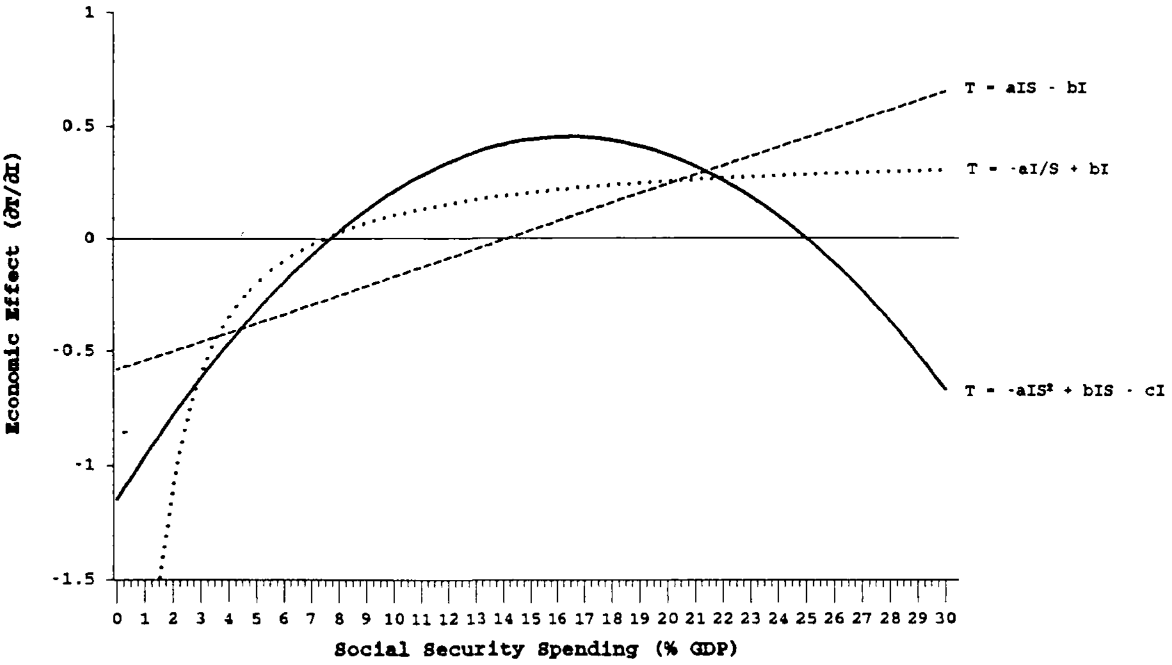

TABLE 3

Turnout and the Economy: Hyperbolic and Polynomial Interaction Models (N = 212)

REGRESSOR	HYPERBOLIC (1)	POLYNOMIAL (2)
INCOME	.403* (.191)	−1.15*** (.303)
INCOME/SECURITY	−3.00*** (.750)	—
1/SECURITY	−.288 (.152)	—
INCOME * SECURITY	—	.196** (.066)
INCOME * SECURITY^2	—	−.006* (.003)
SECURITY	—	.057*** (.015)
SECURITY^2	—	−.003*** (.001)
EXPECT	.906 (.556)	.178 (.110)
NEWVOTERS	.031 (.038)	.020 (.021)
constant	.690*** (.086)	.489*** (.082)
R^2	.817	.848

Note: P (INCOME > 0) > .983; P (INCOME/SECURITY < 0) > .999; P (INCOME * SECURITY^2 < 0) > .993; P (INCOME * SECURITY > 0) > .999; P (INCOME < 0) > .999. Dependent variable is turnout; entries are unstandardized regression coefficients (standard error). Table omits country dummy variables.
*$P \le .05$.
**$P \le .01$.
***$P \le .001$.

TABLE 4

Economic Security Affect on Turnout in the Developed World

REGRESSOR	EXTREME LEVELS OF SPENDING (1)	MODERATE LEVELS OF SPENDING (2)
INCOME	−.274 (.180)	.186* (.081)
EXPECT	.132 (.084)	.580** (.095)
NEWVOTERS	.020 (.036)	−.020 (.028)
Constant	.752** (.215)	.351** (.076)
R^2	.806	.929
N	23	151

Note: Dependent variable is turnout; entries are unstandardized regression coefficients (standard error). Table omits country dummy variables.
*$P \le .05$.
**$P \le .001$.

the hyperbolic or the polynomial is extremely significant ($p < .001$ in both cases).

Which of these is (statistically) preferable is determined by goodness of fit. Based on this criteria, the polynomial is the superior specification, given that the R-squared in this model is greater in statistically meaningful terms than in the hyperbolic ($p < .001$).[10] The preferred specification is thus the polynomial just discussed, where a is the INCOME $\times$ SECURITY^2 coefficient, b is the INCOME $\times$ SECURITY coefficient, and c is the INCOME coefficient.[11]

The adequacy of the polynomial can be tested in another way. If this is the preferred model, then the relationship between the economy and turnout should be essentially constant among subsets of nations with relatively homogenous levels of security spending. Thus, the coefficient of INCOME in the original model (i.e., omitting the security interactions) should vary in a predictable fashion if the analysis is performed on appropriate subsets. It was previously demonstrated that dividing the sample into more- and less-developed nation groups produced a positive coefficient for the former and a negative one for the latter. A further distinction should manifest itself if the more-developed category is further divided into nations with moderate-to-high economic security spending and nations with the highest levels of spending.

Assuming a polynomial relationship, the coefficient should be positive in the former and negative or zero in the latter. A hyperbolic function (or, similarly, a logarithmic) would be indicated by both sets' having positive coefficients. As Table 4 illustrates, the data suggest the polynomial. In performing the regression separately for (1) the states at the extreme of economic security spending (France, the Netherlands, and Sweden) and (2) the remainder of the developed-world sample, the coefficient in the latter is positive and significant as expected, while in the former it is negative in sign though not significant at the conventional .05 level.[12] The failure to reach significance may be the result of a comparatively small sample size or may reflect an actual zero relationship. In either case, the result is congruent with expectations.[13]

EXTENSION TO UNITED STATES DATA

If citizen response to economic conditions is a nonlinear function of the extensiveness of social security provisions, then the same curvilinear pattern should manifest itself not only across countries with widely divergent levels of such programs but also within nations across time as security programs arise and develop. This hypothesis is examined with time series data for the United States from 1896 to the present.[14]

This strategy allows for a comparison with a similar analysis by Rosenstone (1982). Examining elections until 1980, he shows that instability in consumer prices depressed turnout in presidential and midterm elections, with a 5% change in election year prices reducing participation by about 1% (Rosenstone 1982, 40–41).[15] I utilize the same model as follows:

$$\begin{aligned}\text{TURNOUT}(t) = a &+ b1(\text{PRICES}) + b2[\text{TURNOUT}(t-1)]\\ &+ b3[\text{TURNOUT}(t-2)] + b4(\text{MIDTERM})\\ &+ b5(\text{WOMEN}) + b6(\text{YOUTH}) + b7(\text{WAR}) + e,\end{aligned}$$

where TURNOUT(t) is the proportion of the eligible population that voted; PRICES is the absolute value of the election year proportional change in consumer prices;[16] TURNOUT($t-1$) is the proportion voting in the immediately prior national election; TURNOUT($t-2$) is the proportion voting in the penultimate national election; MIDTERM = 1 for midterm years, 0 otherwise; WOMEN = 1 if year is 1920 (advent of women's suffrage) 0 otherwise; YOUTH = 1 if year is 1972 (enfranchisement of 18-year-olds) 0 otherwise; and WAR = $(1/(\text{months at war})^2$ if year is 1918, 1942, or 1944 (to account for lower turnout during the wars), 0 otherwise. The use of the inverse of squared months reflects the fact that the depressive effect of war on turnout declines over time as people accommodate themselves to the accompanying disruption of their lives (see Rosenstone 1982). For purposes of comparison, Table 5, column 1, replicates Rosenstone's result; parameter estimates are essentially identical with those he reports (1982, 40).

Some evidence for the importance of security spending is evident in the instability of Rosenstone's result. Simply expanding the time frame from 1980 to 1988 reduces the coefficient of the economic variable to insignificance ($t = 1.40$), as column 2 of the same table illustrates. The implication is that the significance of the original result depends upon the predominance of elections early in the series. Adding more recent cases dilutes the importance of the earlier elections, leading to a smaller coefficient (both absolutely and relative to standard errors). In effect, the slope of the regression line becomes flatter as more recent elections are added, suggesting that withdrawal has been lessened. This is precisely what one would expect if voter response to economic conditions was mediated by the level of social security commitment in the hypothesized fashion, in that the growth in security spending in recent decades should have eroded the withdrawal effect.

By the same logic, withdrawal should be most pronounced in the middle period between the birth and maturity of the American version of the welfare state. This era is demarcated on one side by the 1935 passage of the Social Security Act and, with it, the establishment of the first federal unemployment, old age, invalidity, and death insurance programs and by the era of expansion that began in 1965 with such new ventures as Medicare, Medicaid, foodstamps, and Aid for Families with Dependent Children.[17] Because there were delays from the actual passage of the legislation to meaningful implementation, I have added a five-year lag period. This approach also factors out the latter years of the Depression, so that the magnitude of the coefficients are not inflated (or otherwise "contaminated") by the extraordinarily large variation in economic conditions that characterized this period (Gough 1984). As Table 5, column 3, attests, the data confirm the expectation of a greater economic impact on turnout in this period. The coefficient of consumer prices is significant and is more than twice as large as the value for the original time frame of 1896–1980.[18]

TABLE 5

Economic Affect on Turnout in the United States By Era

REGRESSOR	1896–1980 (1)	1896–1988 (2)	1940–1970 (3)
PRICES	−.249*	−.200	−.629*
	(.121)	(.143)	(.246)
TURNOUT_{t-1}	.437**	.424**	.489*
	(.081)	(.070)	(.189)
TURNOUT_{t-2}	.252**	.260**	.065
	(.078)	(.097)	(.217)
MIDTERM	−.190***	−.188***	−.241*
	(.024)	(.027)	(.060)
WAR	−8.35*	−8.66*	−8.20
	(3.34)	(3.71)	(4.43)
WOMEN	−.057	−.082	—
	(.031)	(.047)	
YOUTH	−.045	−.039	—
	(.025)	(.041)	
Constant	.274**	.259***	.377***
	(.037)	(.037)	(.092)
R^2	.952	.871	.968
N	43	47	16

Note: Dependent variable is turnout; entries are unstandardized regression coefficients (standard error).
*$P \le .05$.
**$P \le .01$.
***$P \le .001$.

A more rigorous test is to replicate the polynomial model from the previous section for the United States. Unfortunately, reliable data on security spending do not exist prior to 1949. Accordingly, I rely upon an alternative suggested by Cutright (1965) and successfully employed by several others (e.g., Aaron 1967; Jackman 1975; Pryor 1968). This measure is essentially a count of the number of extant economic security programs, weighted by the number of years they have been in operation. The International Labor Organization and the U.S. Social Security Administration index five types of such programs: work injury, sickness or maternity, old age and invalidism benefits, unemployment compensation, and family allowances. (For complete details, see International Labor Office 1985; U.S. Social Security Administration 1984). Thus, for each year a score of one to five is possible, indicating the extensiveness of coverage. While spending data is generally superior in that it represents both depth and extensiveness, the coverage index is highly correlated with spending (Cutright 1965; Pryor 1968) and is generally thought to be "an essentially equivalent measure of policy effort" (Jackman 1975, 15).[19]

The same interaction terms as in the previous polynomial model are added to this specification, subject to the already-discussed caveat that the SECURITY variable is replaced by the social insurance index (INDEX). Note that the expected sign of PRICES is the opposite of that of INCOME: a positive value indicates mobilization, a negative one withdrawal. Accordingly, the hypothesized total relationship between price instability and turnout, at various levels of security, is now of the form $ax^2 - bx + c$ (the mirror image of the relationship with real income). Table 6 indicates the results. The coefficients of PRICES and both interaction terms are statistically significant and of the correct sign. This evidence, coupled with the a priori expectation of this functional form, is strong reason to accept the polynomial as the preferred specification.[20]

DISCUSSION

I began with a test of the notion that turnout varies as a linear function of the state of the macroeconomy. In this formulation, deviations from an expected, or "normal," level of turnout can be expressed by the equation

$$T = aE + b, \qquad (1)$$

where T is deviation in turnout and E is a measure of short-term economic performance. Perhaps the single commonality in the literature is an acceptance, explicitly or otherwise, of this specification. The disagreement is not over the model per se but over the sign of a—positive, negative, or zero.

I have argued that the actual relationship between turnout and the economy is conditioned by an intervening variable in a nonlinear fashion. Specifically,

$$T = -aES^2 + bES - cE + d, \qquad (2)$$

where S is the level of societal commitment to social security and T and E are as just defined. For any given level of security, then, the economic effect can be estimated by taking the first partial derivative of T with respect to E: $\partial T/\partial E = -aS^2 + bS - c$. This quantity represents, in essence, the value of a in equation 1 at a specific level of security spending. As spending changes either temporally or spatially, the economic effect changes, as well.[21]

Still, the question who is affected by the state of the economy remains. In other words, what sorts of citizens are withdrawing or being mobilized? There are three hypotheses: (1) middle- and upper-income groups, (2) the whole population uniformly, and (3) the economically marginal. While the data do not speak directly to this issue, some tentative conclusions may be drawn.

TABLE 6

Economic Affect on Turnout in the United States: 1896–1988 (N = 47)

REGRESSOR	COEFFICIENTS
PRICES	.364* (.220)
PRICES * INDEX	−2.11** (.768)
PRICES * INDEX^2	1.08** (.400)
INDEX	.128*** (.035)
INDEX^2	−.079*** (.018)
TURNOUT_{t-1}	.105 (.084)
TURNOUT_{t-2}	.605*** (.074)
MIDTERM	−.079*** (.024)
WAR	−13.8*** (4.23)
WOMEN	−.122** (.039)
YOUTH	−.029 (.035)
Constant	.236*** (.039)
R^2	.935

Note: P (PRICES * $\text{INDEX}^2 > 0) > .996$; P (PRICES * INDEX $< 0) > .997$; P (PRICES $> 0) > .951$. Dependent variable is turnout; entries are unstandardized regression coefficients (standard error).

$^*P \leq .05$.

$^{**}P \leq .01$.

$^{***}P \leq .001$.

The empirical results I have reported offer strong circumstantial evidence to suggest that hypothesis 1 is wrong, in that the middle and upper classes are the least likely to place much reliance on welfare programs. Hence, the hypothesis leaves little room for the welfare state variables to play a role. If it were these groups that primarily respond to economic conditions, the economic effect on turnout should be essentially uniform across different levels of welfare state development. Given that it is not, this hypothesis seems untenable.

An appraisal of the remaining hypotheses must begin with the observation that the economically marginal are more likely to suffer economic adversity than the middle- and upper-middle classes. Verifying what common sense suggests, Rosenstone (1982) reports that those who were unemployed or described themselves as worse-off financially (on the usual National Election Study [NES] item) were much more likely to be poorly educated, low-income earning persons. Rosenstone also documents that those suffering from these problems are less likely to vote. Hence, even if we assume that economic adversity depresses turnout across all income strata, lower-income groups will be demobilized at significantly greater rates because individuals within these groups are disproportionately likely to face adversity. Thus, even if hypothesis 2 is correct, it will be the economically marginal that, as a group, are most affected. In terms of net results, then, hypothesis 2 reduces to hypothesis 3: oscillations in the business cycle predominantly affect the participation of working-class citizens.[22]

There may also be implications for economic voting and, thus, democratic accountability. While there has been a continuing debate over whether the economic criteria in voter decision calculi predominantly involve personal finances ("pocketbook voting") or assessments of the macroeconomy ("sociotropic voting"), there is now little doubt that the former plays an important (though not necessarily exclusive) role, at least in presidential voting. Using a pooled cross-sectional design and addressing the criticisms raised by Kramer (1983) and others over the inadequacies in the literature, Markus demonstrates that self-reported changes in one's personal financial situation are strongly linked to candidate choices in presidential elections. Hence, those who are worse-off are more likely to vote against the incumbent administration. Using precisely the same NES measure, Rosenstone (1982) demonstrates (as we have seen) that those who report that they are worse off are much more likely to abstain. Thus, given that (1) those personally affected by economic adversity are more prone to vote for the opposition and (2) precisely these same people are also less likely to vote, then, clearly, many of the people who would otherwise have "punished" the government will not turn out.

In assessing the political relevance of security measures in light of these findings, one is forced to consider the radical critique of the American welfare state. Welfare provisions, some critics have suggested, actually contribute to the marginalization of the poor in that such programs reinforce the status quo by preventing economic grievances from being politicized. Although the present study touches this issue only tangentially, the data do provide some evidence for precisely this sort of argument. In the United States, at least, the effect of security programs has been to help insulate the political system from the (electoral) expression of economic discontent. By effectively (and perhaps unwittingly) encouraging such individuals to withdraw from the political process, security programs have reduced their political importance.

On the other hand, the data suggest that the problem is not the welfare state per se but the distinction between "marginal" and "institutional" welfare systems (see, e.g., Korpi 1980). In the former (as exemplified by the United States), security provisions are poorly funded, highly selective, means-tested, and targeted toward specific groups that consequently "suffer stigmatization or resentment" by the rest of society (Ringen 1987). In the latter, epitomized by Sweden, programs tend to be universal, well-funded, and socially integrative (rather than divisive). Economic security is thus both more available and less fraught with social scorn in the institutional type than in the marginal. Thus, those in financial duress find opportunity costs to be higher in the context of the marginal welfare state. Turnout, consequently, is lowered in times of recession. As the institutional ideal type is more closely approximated, withdrawal is lessened and ultimately reversed. In such contexts, security programs not only fulfill their primary purpose of meeting material needs but may also encourage electoral participation in the face of economic hardship. In this fashion, the welfare state may actively contribute to the political empowerment of the disadvantaged.

Lastly, the results suggest that comparative studies of turnout should consider levels of welfare state development. Security programs appear to affect electoral participation systematically. Thus, it may be useful to consider security programs along with other institutional factors that are thought to account for cross-national differences (see Jackman 1987; Lijphart 1984; Powell 1986). In the same vein, more attention might profitably be paid to other directly political consequences of the modern welfare state. While the literature has addressed the sociological and economic results of this phenomenon, relatively little is known about its effect on the political world.

Notes

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1. With the exception of Rosenstone (1982), the aggregate studies generally fail to control for changes in the legal environment (such as the enfranchisement of 18-year-olds in 1972, which depressed turnout) and to factor out secular trends in turnout (due, e.g., to decreased party identification). The survey research also tends to inadequately control for individual-level correlates of turnout (e.g., Fiorina 1978) or to rely on a sample skewed by excluding citizens not registered to vote (Schlozman and Verba 1979). For a discussion, see Rosenstone 1982.

2. The one exception is Lewis-Beck and Lockerbie's (1989) analysis of hypothetical general elections in four West European countries. Using Eurobarometer data, they find that those who see the economy improving in the coming year are more likely than others to vote "if the election were held tomorrow." There are two serious problems with their analysis. First, their economic variable asks for an assessment of "whether, a year from now, the government's policies will

have improved the country's general economic situation." This question clearly taps an evaluation not only of the economy but of the government's management of it. As such, it would seem to be an indicator of partisan attitudes and more appropriate to how one might vote rather than whether one will vote. Second, opinions about one's propensity to vote are likely to be poor predictors of actual behavior, given that the cost–benefit calculations governing the expression of an attitude and the performance of an act differ quite radically.

3. Data are for legislative elections (lower house), except for strong-presidential, nonparliamentary systems. Data for Portugal include the constituent assembly of 1975. The 1983 Jamaican election is excluded due to opposition boycott and other irregularities. Similarly, the 1982 Sri Lankan election is omitted because of both ethnic violence and a dramatic change in electoral procedure. Data were unavailable for the 1986 Costa Rican election and the 1987 elections in Australia, Belgium, Italy, New Zealand, Switzerland, Trinidad, Turkey, and the United Kingdom.

In assembling a list of democratic countries, I have relied on the states identified as such in Butler, Penniman, and Ranney 1981 and Weiner 1987. These include the Western European democracies, Australia, Canada, Costa Rica, India, Israel, Jamaica, Japan, New Zealand, Sri Lanka, Trinidad, Turkey, the United States, and Venezuela. I have also excluded Colombia, on the grounds of both the extremely limited competition that occurred during the National Front era and the more recent problems of electoral violence. Malaysia, too, had to be omitted, because of the (arguably) limited competition characterizing its electoral system, as well as a lack of reliable data. The Dominican Republic is also excluded due to lack of reliable data. For the remaining countries data are generally acknowledged to be reliable.

4. The use of lagged data carries with it the potential for violations of the assumptions regarding the error terms. To account for this, all equations were also estimated with a generalized least squares model that controls for serial correlation. However, since the parameter estimates in this model did not differ meaningfully from those derived from ordinary least squares, I have reported the results from the simpler procedure.

5. Developing countries are Costa Rica, India, Jamaica, Sri Lanka, Trinidad and Tobago, Turkey, and Venezuela. While both Trinidad and Venezuela score in the same range as poorer European countries such as Portugal on some measures of economic development (e.g., per capita gross national product), the values for the former are artificially inflated by oil revenues. Further (as will be argued), the critical distinction between developed and developing worlds appears to be one of welfare state development. On such a measure, Venezuela and Trinidad belong squarely with the other developing countries.

6. Following Powell (1980, 1986) and Jackman (1987), I calculate turnout as the ratio of total (valid plus null) votes to the total eligible population. The eligible population is calculated as suggested by Jackman (1987, 419–21). Vote data is from Mackie and Rose 1982; Weiner and Ozbudun 1987; *The World Atlas of Elections, Elections on File*, and various years of the official statistical yearbooks of India, Jamaica, Trinidad, and Turkey. In a few cases, it is not entirely clear whether the reported figures represent total votes or merely valid votes. However, differences between the two tend to be very small, and the data are consistent for each nation.

7. The economic data are from the World Bank. A few missing cases have been filled with data from various editions of the United Nations *Statistical Yearbook*. While data from the two sources are not strictly comparable, there is no overlap—i.e., all economic data for a single country for a given year are from a single source. The election year time frame is traditional in the economic voting literature (e.g., Kramer 1971; see also Rosenstone 1982). Unfortunately, since only annual (rather than quarterly) data is available for much of the sample, economic change can be measured only in calendar years. This presents no difficulty as long as the election is held late in the year, but it is troubling for early elections. Hence, for contests in the first part of the year (before June), I have used change in the year prior to the election year.

8. While the institutional arrangements structuring voting remained essentially constant in most countries during the time frame in question, there were a few instances of important changes (e.g., the elimination of mandatory voting in the Netherlands in 1970). I experimented with fitting dummy variables for these events, as well, but dropped them, given that their inclusion did not affect the results. Note that the impact of these changes, as well as those of an expanded electorate, become absorbed into the EXPECT variable for later elections.

9. An additional statistical (rather than substantive) test of the predicative utility of the additional variables is to determine whether the increase in R-squared with their inclusion is statistically greater than the simpler model that excludes them. The difference between the R-squared reported in Table 2 for the expanded model and that which occurs when omitting the extra variables (i.e., for the basic model when examining the entire sample; details not shown) is significant at the .05 level ($F = 3.72$).

10. For the first order (Table 2) versus hyperbolic (Table 3, col. 1), $F = 21.6$. For the first order (Table 2) versus polynomial (Table 3, col. 2), $F = 83.3$. For the polynomial versus the hyperbolic (Table 3, cols. 2 and 1, respectively), $F = 19.2$. For a description of these tests, see Cohen 1968. Note that while the difference in explained variance between the polynomial and the hyperbolic is modest, it is highly significant. The relatively small increase is attributable to the fact that two functional forms are geometrically similar for most of the range of the data. It is only at extremely high levels of security spending that the fitted curves begin to diverge (see Figure 1). There are very few data points at this extreme, making it difficult for the proper specification to dramatically alter R-squared.

11. The use of multiple interaction terms does introduce the prospect of multicollinearity. The practical effect of this phenomenon is to inflate standard errors, so as to bias parameter estimates toward insignificance. However, when such (potential) inflation is not sufficient to interfere with the test of the relevant hypotheses (the signs of the coefficients), "then collinearity has caused no problem" (Belsley, Kuh, and Welsh 1980, 116). In the present application, the parameter estimates are quite large relative to their standard errors, so that there is little question as to their correct sign. For the terms of interest in equations where collinearity is a potential problem (the equations reported in Tables 3, 5, and 6), the probability of each coefficient's being the correct sign is .95 or larger. (See relevant tables for details.) Note also that the reported result is symmetric about the sign of INCOME, i.e., the same substantive implications obtain when dividing the sample into INCOME > 0 and INCOME < 0 subsets. Hence, both growth and recession affect turnout.

12. Still, the probability of INCOME's being of the correct sign is about .93. Note also that including additional countries with extreme social security spending (e.g., Denmark and Germany) produces the same substantive result.

13. Additional evidence for a (quadratic) polynomial form can be found by including the first-order INCOME = SECURITY interaction in each of the developing and developed world subsamples. In the former, the INCOME coefficient is negative and the interaction positive, indicating mobilization that gives way to withdrawal as security spending increases. In the latter, INCOME is positive and the interaction negative, indicating withdrawal that becomes mobilization with sufficient spending. If the true form is, in fact, a quadratic, then dividing the sample in this fashion and utilizing a linear estimation procedure would produce exactly this result.

14. The United States provides an ideal test case in that it has an electoral time series that is both long and dense. Other countries provide greater temporal variation in security spending but are hampered by a less-dense time series. The United States has constitutionally mandated biennial elections, leading to a far greater number of data points than

parliamentary systems with variable (and generally longer) interelection periods. Further, while the across-time range of security spending is larger in many other countries than the United States, effective variance relative to electoral data is less, given that substantial progress in the development of the welfare state was generally obtained much earlier in these nations than in the United States.

15. Turnout data are derived from Burnham's figures in *Historical Statistics, Colonial Times to 1970* for the period 1896–1934 and *Historical Statistics of the United States* (U.S. Department of Congress, 1990) from 1935 to the present. Consumer prices are from the same sources. To account for slight differences between the series, change in prices are determined for any given year entirely from a single source. Similarly, lagged turnout values are taken from the same series as the current value.

16. Note that PRICES is not strictly equivalent to the rate of inflation, in that it represents the *absolute value* of changes in prices and thus measures not inflation but instability in consumer prices, regardless of direction. Rosenstone (1982) introduces this term as a surrogate for two indicators that he found related to turnout: short-term unemployment and proportion of the population reporting itself worse off financially. Neither of these direct measures of economic adversity are available prior to 1948, which makes them clearly inappropriate for a time series analysis. I have followed Rosenstone in the use of this variable, in order to compare my results explicitly with his. However, the same substantive implications follow when using election year proportion change in per capita real income in its place.

17. A handful of states introduced unemployment schemes in the period shortly prior to the federal legislation of 1935. Prior to this period the only significant governmental programs concerned only work injury and were administered (and financed) by the states (along with a national plan applicable only to federal employees).

18. The result does not depend upon the selection of these precise dates as cutoff points. Similar findings obtain when using other points, such as 1935 and 1965.

19. The items comprising the index also form a Guttman scale, i.e., index scores genuinely represent a continuum of social insurance development, so that higher scores uniformly represent greater development than lower scores. (For a discussion, see Cutright 1965.) Note that I have actually used the index value divided by one hundred, so that the resulting parameter estimates will be of the same general order of magnitude as for the other variables. Note also that the same substantive results reported for the cross-national sample are also derivable from utilizing the social insurance index, instead of the spending variable. I have utilized spending data where possible because such data are more readily interpretable.

20. The increase in R-squared (adjusted for loss in degrees of freedom) is significant, since both the first- and second-degree interactions are sequentially introduced ($p < .001$ in both cases). Thus, the polynomial interaction is statistically superior to both the basic model and that containing only the simple PRICES × INDEX interaction. Note also that reestimating the model in Table 6 using real income instead of PRICES (and keeping the model otherwise unchanged) produces the same substantive implications.

21. Note that these values are, of course, precisely those derivable from the regression equation in Table 3, col. 2. Using this information, the maximum level of withdrawal is easily determined by setting the value $\partial^2 T/\partial S \partial E$ to zero and solving for S. The result indicates that this occurs at a security spending level of 16.1% gross domestic product, such that a 5% decline in INCOME translates into a 2% decline in turnout.

22. In fact, it seems unlikely that uniform magnitude would obtain, in that upper-income households will tend to have greater ability to withstand adversity than lower-income households. As Weatherford puts it, the working class is a "descendant group—one whose income opportunities decline faster than the overall average during a recession, and one which can less afford to cushion the decline with accumulated savings, property, etc." (1978, 922–23). Thus, not only will the proportion of working-class individuals affected by economic adversity be larger than that of the middle and upper-middle classes, but such adversity also will tend to have a greater affect on the former as individuals. Hence, at both the individual and group levels, financial difficulties should have a disproportional impact upon the turnout of the working class.

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RESEARCH NOTES

COMMUNITY VERSUS MARKET: A NOTE ON CORPORATE VILLAGES

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We return to the literature on collective villages and reexamine its central arguments. In doing so, we focus on an institution for allocating land that we call the Rule. As claimed by its advocates, the Rule secures land allocations that result in outcomes differing from those that would be achieved by markets. The outcomes are not constrained to be efficient. But the economic costs paid fail to secure the attainment of guarantees of subsistence, greater equality, or a greater sense of community resulting from the elimination of envy. However, in the face of high levels of risk, the form of collective property rights embodied in the Rule may create what we call "communities of (mis)fortune," in which no one can envy others' gains or losses resulting from the random shocks of nature.

Barrington Moore (1966) identified the peasantry as the revolutionary class of our time. The power of guerrilla forces in Vietnam rapidly convinced scholars of the validity of Moore's claim. Particularly when viewed from a developmental perspective, the revolutionary behavior of peasants posed deep analytic problems, however. Why should members of what arguably is a precapitalistic social formation stand at the forefront of modern revolutions?

Scholars offered competing answers to this question: dependency theory,[1] classical Marxism,[2] moral economy,[3] and rational choice.[4] Among the most heated of the resultant debates was that between the moral economists and the rational choice theorists. We reenter that debate, seeking to extend and deepen it by formalizing its central arguments.

Moral economists based their explanations of peasant revolution on an analysis of rural institutions, arguing that key peasant institutions (e.g., the corporate village) create collective, as opposed to private, property rights. They therefore support the attainment of values often sacrificed by markets: equality, community, and (above all) the right to subsistence. When market economies penetrate peasant societies, then, peasants react with moral outrage. Their defensive rejection of incorporation into the modern world economy provides a radical thrust to political protest in agrarian societies.

Rational choice theorists have attacked this argument on both empirical and logical grounds. For example, Popkin challenges Scott's depiction of the internal politics of corporate villages and demonstrates that the consequence of peasant revolution does not follow from the premise of peasant values, given the incentives to free-ride (Popkin 1979).

We focus on the logic of the arguments put forward by the moral economists. Our critique is formal and deductive; it isolates and examines the properties of an institution that we call the Rule. We aim our attack at the foundations of the moral economists' argument: the link between peasant institutions and values. In particular, we show that an archetypical institution of peasant society—the corporate village with collective rights in property—cannot logically provide a defense of the core values of peasant society: equality, community, and the guarantee of subsistence. The implication, of course, is that the moral outrage that fuels peasant revolutions must originate elsewhere than from the breakdown of this institution.

Although there are many other peasant institutions, (e.g., lineages, reciprocity, patron–client ties), other, highly complex forms of property rights, and other sources of peasant revolution, claims about what we call the Rule lie at the foundation of the moral economy argument. Hence, we have chosen to model that feature.

THE MORAL ECONOMY ARGUMENT

Central to the literature on peasant rebellions are studies of corporate villages. Corporate villages limit individual rights in property. The community restricts the rights of individual families to purchase, lease, alienate, rent, or use land. In some cases, corporate villages enforce forms of communal tenure. Land rights then remain fully vested in the community, and the village distributes the land among its members. In such cases, the corporate village may be called a repartitional commune.

While apparently relatively rare, corporate villages and repartitional communes have long attracted great attention. At least since the mid nineteenth century, scholars have written extensively about these forms of villages and made strong claims on their behalf.[5] Most recently, they have played a central role in the moral economy school of peasant behavior (Migdal 1974; Scott 1976; Wolf 1973).

The Russian *mir* constitutes perhaps the most famous example of such an institution. We shall draw our "stylized facts" from the literature on the *mir*. We focus upon several features that loom large in the scholarly debates, which provide the subject of our analysis. At any given time, these features may have characterized some village communities but not others; or they may have been typical of the *mir* in one period but not in another.

The *mir* consisted of a collection of families who gained rights and incurred obligations through membership in a village. The most important right was to share the village's land, while the most important obligation was to share in the burden of its taxes. As the literature on the *mir* makes clear, the two were connected.[6] The rural community was assigned a tax bill by the state, the size of which was proportional to the number of the village's souls (a soul being a working-age person).[7] To meet its obligations to the state, the village then divided the tax bill among its member families while also apportioning them a share of the village's land—and thus the ability to pay the taxes.

Throughout, we will focus on the rules that governed the partitioning of village lands. Assuming the village to possess different kinds of qualities of land K_1 and K_2, the Rule assumes the following form: if a family i contains $L^i/L = C^i\%$ of the community's souls then that family receives $C^i = K_1^i/K_1$ of the village's land of type 1 and $C^i = K_2^i/K_2$ of the village's land of type 2, where L is the number of souls in the village, L_i is the number of souls in the family, K_1 and K_2 are the number of acres of type 1 and type 2 land in the village, and K_1^i and K_2^i are the amount of each type of land given to family i. In our model, type 1 land is of a better quality than type 2.

Discussions of collective villages stress the high levels of risk that confront agrarian societies. Rains can fail; disease can infect crops, livestock, or human beings; and prices can change, lowering the purchasing power of rural households. Given their small endowments and marginal physical and economic locations, rural families facing such shocks can starve. The possibility of the loss of subsistence constantly confronts the inhabitants of poor, agrarian societies.

Noting these characteristics of the institutions and environment of collective villages, scholars develop a powerful line of argument. Their basic contention is that collective villages are social—as opposed to economic—institutions. By this they mean that the allocation of factors of production in collective villages is determined by social relationships rather than by markets.[8] Land is not bought and sold; it is allocated by the community. Nor is labor a commodity to be exchanged in markets; rather, peasants draw their labor from their families. Markets thus play a limited role in the allocation of factors of production; the allocation is, instead, structured by social institutions.

From this premise, the contributors mount their first claim—that rural economies governed by collective villages generate outcomes that differ from those of markets. In particular, they do not allocate resources so as to maximize total output. The allocation of resources in such societies is not constrained to be efficient.[9] A second claim is that collective villages constrain the allocation of resources so as to enable rural societies to secure certain key values—presumably values threatened by the kind of behavior necessary to achieve the maximization of the total social product. The most important of these other values is the *security of subsistence*. Polanyi writes, "It is the absence of the threat of individual starvation which makes primitive society, in a sense, more human than [the] market economy, and at the same time less economic" (1957, 164). Characterizing the writings of Scott, Wolf, and others, Popkin notes: "Village procedures reflect this overriding concern of peasants with survival. . . The 'right to subsistence' is the primary normative concern" (1979, 11). Another important value secured by collective villages is *justice*. While not rigorously defined, the term is used in ways that imply equity and a concern with sharing. The village is a community of fortune, with no one able to control resources in ways that permanently privilege their life chances.[10]

In short, collective villages, based upon the division of the land among *all* families, are held to be "moral economies"[11]—in contrast to market economies, which tolerate inequality and where a person, if poor, can starve for want of access to productive resources.

CRITIQUE

As might be expected, the communal allocation of land generates allocations that represent an alternative to the outcome secured by markets, allocations that are not efficient. Contrary to what is also claimed, however, the losses in production do not generate benefits in terms of enhancing the attainment of social values in the shape of security of subsistence, equity, or the elimination of envy. The justification for the collective allocation of land must lie elsewhere.

The Set-Up

We assume that there are no markets for land or labor, land being allocated by the community and producers employing family labor; and that peasant production functions are concave.[12] We also assume the existence of two forms of taxation:

1. *Tax rule 1*. Families are taxed according to the number of souls, that is, working-age adults in the family.
2. *Tax rule* 2. Families are taxed according to the size of their land holdings.

Lastly, we omit from our analysis cases in which the community's tax bill exceeds its total product. We thus rule out cases where tax rule 1 is in effect but where the land is scarce or of very poor quality. We also rule out cases where, under tax rule 2, the receipt of additional land imposes losses upon families. In the absence of hired labor, the acquisition of additional land by a family can lead to a decline in the marginal product. Under tax rule 2, at some point, the marginal product of an additional unit of land could thus be less than the increase in the tax bill resulting from its acquisition; land would then become an undesirable good. We shall assume that neither situation holds, although Wallace confirms that such situations did in fact occur in practice (1961, 283). The result is to bias the analysis in favor of the arguments in the literature, making our criticisms more telling.

Efficiency

Let there be two types or qualities of land, K_1 and K_2. Applying the Rule, each family i then receives a share of each type of land proportional to its share of the total village population; that is, if the relative size of family i is $C^i = L^i/L$, then it receives $C^i = K_1^i/K_1$ of type 1 land and $C^i = K_2^i/K_2$ of type 2.

If we assume that production on land of each type is Cobb–Douglas, then family i's production function can be written

$$Q^i = (K_1^i)^{\alpha_1}(L_1^i)^{\beta} + (K_2^i)^{\alpha_2}(L_2^i)^{\beta},$$

where K_1^i and K_2^i are the family's allocation of each type of land and L_1^i and L_2^i are the family's allocation of its labor L^i $[L^i = L_1^i + L_2^i]$ across its holdings of the two types of land.[13]

Allocating its resources optimally, each family i will shift workers between plots until the marginal product of labor is equal on each type of land. But when individual families behave rationally and maximize their total production, the result is not "socially rational" in the sense of maximizing the total product of the village community. To see this, note that seeking to optimize, each family will devote labor to its land of type 1 until

$$\left(\frac{L^i - L_1^i}{L_1^i}\right)^{1-\beta} = \frac{(K_2^i)^{\alpha_2}}{(K_1^i)^{\alpha_1}}\,.$$ [14]

For a given family, the optimum use of labor on land of a given type is thus a function of its *total endowment of land of all types*. Given that land is allocated among the different families according to the Rule, the size of this endowment will vary among families.

The community allocates land so as to equalize the land–labor ratio across families for each kind of land; each family, behaving optimally, equalizes the marginal product of its labor on the two kinds of land. The result is that the community's allocation does not yield behavior by families (even though they optimize) that produces marginal products of land and labor that are identical for all families. Given that labor is not traded, we cannot expect subsequent reallocations of labor to equalize the marginal products of land among the village families. And given that land is not traded, we cannot expect subsequent reallocations of land to equalize the marginal products of family labor. The first-order conditions for the maximization of the total community product, therefore, would not hold, given the Rule, even though each peasant family might allocate its own land and labor so as to maximize the total family product.

The implication is that one of the claims of the moral economy is correct. Imposing rules to equalize the allocation of land per laborer, communal villages do indeed achieve outcomes that fail to maximize the productive use of their resources. As claimed in the literature, such communities abide by principles other than those embodied in the market. A key question then becomes whether (as is also claimed) the resultant allocation, while economically costly, is socially desirable, in that it supports the attainment of noneconomic values.

Subsistence Guarantees

A key value that communal institutions are supposed to provide is the guarantee of subsistence. However, the rule of allocating land to a family in proportion to the number of souls fails to provide that guarantee. Most obviously, in a bad year the Rule could not guarantee subsistence: guarantees of access to land would not guarantee access to output; for with natural calamities, production could fail. Under such circumstances, few systems could succeed. In fairness, then, the Rule should be subject to a less demanding test.

One test is the ability to underpin the subsistence needs of less fortunate families, that is, those with low marginal products. We can define subsistence needs as $sn + t$, where n is the number of mouths to feed, s the subsistence requirements of each "mouth," and t the tax bill. It is possible, of course, that members of a family could be so incapacitated that no amount of land, if operated by its members, could yield sufficient output to meet its subsistence needs.[15] It is also possible that the marginal product of labor of the family could be so low that given the increase in its tax bill (under tax rule 2) associated with the acquisition of additional land, the subsistence needs of the family would be imperiled by the receipt of greater amounts of land.[16] Even apart from such problems, there remains an additional difficulty: the lower the productivity of a family's labor, the greater the amount of land it requires to meet its subsistence target. Should the community seek to provide unfortunate families with sufficient land to meet their subsistence needs, it would have to reduce the amount of land supplied to other families. In seeking to guarantee the subsistence needs of its unfortunate families, in other words, it would necessarily place the subsistence needs of others at greater risk.

To illustrate, we once again assume that family production functions are Cobb–Douglas. Then, solving for the maximum possible total output yields an expression for the conditions necessary to guarantee subsistence:

TABLE 1

Applying the Rule: Equity, Envy, and Guarantee of Subsistence

FAMILY	MOUTHS (1)	WORKERS (2)	ACRES (3)		ACRES PER WORKER (4)	
			TYPE 1	TYPE 2	TYPE 1	TYPE 2
Family 1	10	3	6	12	2	4
Family 2	5	2	4	8	2	4

	TOTAL FAMILY PRODUC. (5)	PER CAPITA CONSUMP. (6)	TAX BILL (7)	SUBSIST. NEEDS[a] (8)	MARGIN OVER SUBSIST.[b] (9)
Family 1	5.6	.56	2.7	6.7	−1.1
Family 2	3.9	.77	1.8	3.8	.1

Note: This example assumes that the production $Q = K_1^{.5}L_1^{.5} + K_2^{.3}L_2^{.5}$, where $L_1 + L_2 = L$ is the number of workers. It also assumes that $s = .4$, $t1 = .3$, and $t2 = .15$, where $t1$ = tax per soul and $t2$ = tax per acre; the values are those necessary for the village to pay a total tax bill of $4.5 thousand.

[a] Defined as $sn + t$

[b] (5) − (8)

$$L_i^{\beta}(K_1^{i(\alpha_1)/(1-\beta)} + K_2^{i(\alpha_2)/(1-\beta)})^{1-\beta} \geq sn + t \qquad \text{for every i.} \qquad (1)$$

Clearly, families with low values of L^i would require larger quantities of K_1^i and K_2^i. If

$$\sum_i K_1^i > K_1 \quad \text{and/or} \quad \sum_i K_2^i > K_2$$

for every possible combination of K_1^i and K_2^i satisfying the above inequality, *then the requirements for guaranteeing the subsistence needs of all families cannot be met.*[17]

The Rule, then, does not, in general, provide subsistence guarantees. It can do so only under specific conditions, such as those just specified. It is important to note that these conditions are more restrictive the poorer the village community: the lower the quality of the land, or the poorer the quality of the labor, or the higher the tax bill, the smaller (according to expression 1) the margin over and above subsistence needs. The poorer the community, in short, the lower the margin left for it to secure the subsistence requirements of its least productive members while not imperiling the needs of others. Hence, applying the Rule cannot guarantee subsistence. In fact, it can increase the degree to which subsistence is threatened. And this threat becomes more severe, the poorer the rural community.

Other Values

By reducing the efficiency of land use, corporate villages impose economic costs. We have argued that these costs are not offset by the major benefit often claimed for them, namely, that unlike the market, corporate villages can guarantee that rural dwellers will not starve. In this sense, we can reject the claim that this alternative to the market is "more human" even though "less economic" (Polanyi 1957, 164).

Perhaps, then, the justification of communal villages lies in their ability to secure allocations that support the attainment of other values: the promotion of equality, the reduction of envy, or the strengthening of the potential for community. In exploring this possibility, we employ an example (Table 1). The example consists of a village containing two families living on 30 acres of land. One family has twice as many mouths to feed as the other: 10 for family 1, 5 for family 2. The families also differ in the number of souls (working-age persons): three for family 1, two for family 2. The village lands are of two types. The village is liable for a total tax bill of $4.5 thousand, which it assigns to its member families either by the number of souls belonging to the family or by the amount of land that the family farms.[18] Applying the Rule, the village then allocates to the two families shares in each type of land, the size of the share depending on the proportion of the village's souls who are members of the family. Family 1 therefore receives 6 acres of land of type 1 and 12 acres of land of type 2. Family 2, having two-thirds of the number of workers as family 1, receives 4 acres of land of type 1 and 8 acres of land of type 2 (Table 1, col. 3).

We can now examine the impact of the Rule upon equality, envy, and the attainment of subsistence. We assume a Cobb–Douglas production function with constant returns to scale, with arbitrarily selected parameter values. The validity of this counterexample rests solely upon the assumption of concavity, however.[19]

From column 4, it is clear that applying the Rule equalizes access to the means of production. As a consequence, neither family could envy the other's land endowment. Columns 5 and 6 remind us of an elementary but important truth, however: equality of land endowments fails to guarantee either equality of total product or equality of consumption. Family 1 contains more workers, gets more land, and therefore produces more output; family 2 could therefore envy its neighbor's production. But family 1 also possesses more members; its members would therefore envy the consumption of members of family 2, who, having a lower ratio of consumers to producers, consume more per capita, even with their lower total product.

The general point is obvious: producing equality in one aspect of economic life, the Rule generates inequality in another. Insofar as the attainment of community requires the reduction of envy, the Rule, therefore, fails to provide the institutional foundations for a community in rural life.

In recognition of this point, some—especially James Scott (1976)—stress the significance of envy in rural life. They emphasize the jealousy faced by those who prosper and the pressures mounted upon the prosperous to divest themselves of accumulated assets by sponsoring public festivals and private clients. Insofar as these behaviors characterize the life of communal villages—i.e. villages governed by the Rule—they underscore the failure of this institution to promote community by reducing envy.

The example illustrates a last point, one made in the previous section. As seen in column 9, the Rule fails to equalize subsistence risk, much less to guarantee subsistence. It gives family 1 insufficient land to feed itself and pay its taxes.

AN ALTERNATIVE INTERPRETATION

Alternatively, we argue, constraints placed on the initial allocation of land could be construed as providing not guarantees but, rather, insurance.[20] The Rule could provide a form of insurance that equalizes the subjective impact of the variability of nature, thereby rendering villages communities in the sense of communities of (mis)fortune.

Consider Table 2.[21] Nature provides good years and bad. There are two types of land. Type 1 does better than type 2 in good years but worse than type 2 in bad. The application of the Rule ensures that every family will hold the two types of land in the same proportion; that is, for families i and j, $K_1^i/K_2^i = K_1^j/K_2^j = K_1/K_2 = \gamma$. As suggested in Table 2, the production achieved by a family depends upon how much of each type of land it holds and whether the year is good or bad. If good, then family i's production will be $4K_1^i + 3K_2^i$, which (given that $K_1^i = \gamma K_2^i$) can be rewritten as $K_2^i(4\gamma + 3)$. Reasoning in a similar fashion, family i's production in a bad year will be $K_2^i(\gamma + 2)$.

TABLE 2

Payoffs Under Given States of Nature and Types of Land

TYPE OF LAND	STATES OF NATURE	
	GOOD YEAR	BAD YEAR
Type 1	4	1
Type 2	3	2

The consequence of equalizing the proportion of the two types of land in the endowments of each family is that should a bad year occur, each family experiences the same regrets.[22] For each family i, the difference in the production between a good year or bad is $K_2^i(4\gamma + 3) - K_2^i(\gamma + 2) = K_2^i(3\gamma + 1)$, yielding a percentage loss of $(3\gamma + 1)/(4\gamma + 3)$. In the absence of the Rule, γ would differ across families. Each family i's percentage loss in a bad year would then be $(3\gamma^i + 1)/(4\gamma^i + 3)$; and because γ^i varies across families γ^j, each family would experience different levels of regret under unfavorable states of nature.

The equalizing impact of the Rule upon land endowments therefore equalizes regrets in the face of unpredictable nature. This result represents a "positive" finding and, indeed, is the first positive result of this analysis. It should be noted, however, that it generates an interesting contradiction.[23]

We have already demonstrated that the Rule, while guaranteeing equality of endowments, cannot guarantee equality of production (or consumption). Therefore, by generating equal percentage losses (or gains) in the face of random shocks by nature, the Rule preserves these inequalities. Once ranked by production levels, when shocked by variations in nature, peasant families subject to the Rule would find their ranks preserved; for they would experience equal proportional gains or losses. We thus encounter a contradiction: the Rule equalizes regrets but at the expense of preserving inequalities in production and consumption.

One response is deeper skepticism about the claims in the moral economy literature concerning peasant equality. A more charitable response (which, indeed, we favor) is to garner an insight into peasant attitudes toward equality. Inequality is tolerable in agrarian societies so long as no family can gain relative to another as a result of the random acts of nature.

CONCLUSION

Claims about peasant society lie at the core of theories of peasant revolution. They also lie at the heart of debates between competing schools of scholarship. In particular, debates between the moral economy and rational choice theories of peasant revolution focus on one of the central institutions of peasant society, the corporate village.

We have returned to the literature on corporate villages, abstracted an idealized representation of a core property of that institution (the Rule), and examined its ethical features. We have found that (as claimed by their advocates) villages applying the Rule secure land allocations that result in outcomes differing from those that would be achieved by markets, namely, inefficient outcomes. But we find reasons to contest other claims made by the moral economists.

The economic costs incurred by using the Rule do not secure the attainment of other values, such as guarantees of subsistence, greater equality, or a greater sense of community resulting from the elimination of envy. In the face of high levels of risk, however, the form of collective property rights embodied in the Rule may create communities of (mis)fortune, in which no one can envy the gains of others resulting from random shocks of nature.

Our formal exploration of central arguments in the peasant literature yields two further lessons. The sense of moral outrage that impels peasants to violence cannot result from the breakdown of institutions based upon conventions resembling the Rule; for we have shown that such institutions cannot underpin the realization of the social values that provide the radical impetus to revolution. In light of this finding, the analysis also underscores the danger of basing theories of peasant revolutions on idealized images of self-contained peasant communities.[24] Theories of peasant revolution might better be based on the analysis of conflicts between peasants and "external" agents, such as exploiting classes or extracting states. In this sense, our analysis suggests explaining peasant revolutions by relying less on studies of the internal culture of peasant communities and more on studies of their external political and economic relations.

Fruitful extensions of our theory would include comparative analysis. The behavior of the Rule should be compared to that of the market. For given distributions of land and compositions of households and given distributions of random interventions by nature, the Rule might well secure socially desirable outcomes with a greater likelihood than the market. Our negative findings might appear less damaging, then, when subject to comparative analysis. Similarly, our investigation of the Rule should be replicated for other peasant institutions, such as lineages (Bates 1990; Posner 1980); common pool resource systems for water, forests, and grazure (Ostrom 1990); and share tenancy contracts (Stiglitz 1986). Only in this way can we fully assess the implications for the claims by the moral economists. Whereas one might argue that should the claims not hold for something as rigorously egalitarian as the Rule, then, a fortiori, they are unlikely to hold for other peasant institutions, another might reply that while the claims might not hold for this particular, highly stylized version of the Rule, they may hold for other institutions. We cannot resolve the matter, then, until we analyze these other institutions.

In addition to comparative analysis, we need to investigate the Rule itself more deeply. In what sense might it represent a "best response" by the members of a peasant community to a government-imposed tax? If, under the Rule, poor people could not advance relative to rich (given random acts of nature) why, then, would the Rule be chosen by a peasant community? We need answers to such questions.

Notes

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1. De Janvry (1981) offers a sophisticated version. More representative would be the interpretive sections of Barnett and Njama 1966 or Palmer and Parsons 1977.

2. Lenin's *Development of Capitalism in Russia* (Lenin 1964) stood as parent to this tradition, just as his *Imperialism* (1924) stood as parent to the dependency school. Some of the most interesting studies of rural accumulation and class formation come out of Africa. See, e.g., the contributions to the peasant debate in *Review of African Political Economy* 20 (1981).

3. Most notably, perhaps, Migdal 1974; Scott 1976; and Wolf 1957, 1973.

4. Most notably, Popkin 1979; see also Bates 1976.

5. See, e.g., the debates between the backers of Baron Haxthausen and B. N. Chicherin, summarized in Blum 1961, 508 ff.

6. This discussion draws on Blum 1961, 1971; Robinson 1949; and Wallace 1961.

7. Occasionally, an adult couple was defined as one soul.

8. The most notable development of this argument is that of Polanyi 1957. An influential amplification and elaboration is contained in Dalton 1971. See also Migdal 1974; Scott 1976; and Wolf 1957, 1973.

9. In typically vivid language, Eric Wolf distinguishes between collective villages and market economies: "Capitalism cut[s] through the integument of custom, severing people from their accustomed social matrix in order to transform them into economic actors, independent of prior social commitments to kin and neighbors. They [have] to learn how to maximize returns and how to minimize expenditures, to buy cheap and to sell dear, regardless of social obligations and social costs" (1973, 279).

10. For a vivid recreation of the power of this ethic, see Hinton 1972.

11. The phrase is, of course, Scott's (1976).

12. We employ a Cobb–Douglas production function throughout. In the extended example (Table 1), we impose an even more restrictive assumption, namely, that production functions are not only Cobb–Douglas but also possess constant returns to scale. We make these assumptions in order to generate numerical results. Our arguments, however, unlike our illustrations, depend not upon the specific form of the production function but solely on its being concave.

13. Note that we are assuming that the form of the production function is similar on each type of land. This might not be the case if, say, one type of land were used for arable production and the other for pastoral. See also n. 12.

James Scott, in a personal communication, indicates that the communal villages of Russia re-allocated land and labor that were "slack," with the result that the process of production in practice approximated the pattern that would prevail, had inputs been allocated through factor markets.

14. This result does not depend upon the assumption of maximizing behavior by the individual families. Using a Chayanov-type production function for the peasant household, for example, would only strengthen the result (Thorner, Kerblay, and Smith, 1966). Secondly, there is some evidence that following the initial allocation of land in accordance with the Rule, farm families did exchange portions of their land holdings (Robinson 1949, 112). These transactions, however, appear to have been limited and unauthorized and not to have fundamentally altered the initial allocation.

15. That is, the total product secured by a family, even with infinite amounts of land, would lie below $sn + t$.

16. As noted by Robinson under tax rule 2, such families would be placed at risk by being given more land (1949, 274).

17. Note that allowing laborers to have varying productivity levels will not alter this result.

18. In practice, given that the Rule allocates an amount of land to each family in proportion to the number of souls, the tax bill assigned each family is identical under the two tax systems (see col. 7).

19. The choice of specific values for the parameters of the production function are arbitrary. So, too, is the choice of a numerical value for s. The values of $t1$ and $t2$ are determined once the number of souls, the quantity of land, and the size of the tax bill are given.

20. The locus classicus of this approach remains McClosky 1976. For a critique, see Dahlman 1980. See also the very interesting work of Quiggin 1988.

21. The argument follows if (as would be natural in the context of the problem), the following general relationships hold: the cells of Table 2 are labeled a, b, c, and d (left to right; a, b in top row; c, d in bottom row), and $a > b$, $c > d$, $a > c$, and $d > b$.

22. It is important to stress that we do not mean *regrets* in the same sense as in decision theory. Our agents, after all, have no choices and so cannot regret their choice of strategies. Rather, by *regrets* we mean losses by comparison to what they could have received in a good year.

23. Note, as well, a less charitable interpretation, namely, that the Rule prevents a shift in the relative rankings of the village families, measured in terms of output, as a consequence of a random shock from nature.

24. It thus joins the criticism of the political economists who criticize traditional anthropological studies of peasant villages, treating them in abstraction from broader political and social forces. See, e.g., the critique contained in Wolf 1973.

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IDEOLOGY, STATUS, AND THE DIFFERENTIAL SUCCESS OF DIRECT PARTIES BEFORE THE SUPREME COURT

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A substantial literature on lower federal courts and state courts suggests that the "haves" usually come out ahead in litigation because they possess superior resources for it and they reap advantages from their repeat player status. We investigate the success of 10 categories of litigants before the Warren, Burger, and Rehnquist Courts to determine whether the resources or experience of litigants has effects on Supreme Court outcomes paralleling those found in the courts below. While different categories of litigants are found to have very different rates of success, those differences do not consistently favor litigants with greater resources. A time series analysis of the success of different categories of litigants over the 36 years studied suggests that the changing ideological complexion of the Court has a greater impact on the success of litigants than differences among litigants in resources and experience.

We examine the impact of litigant status and the changing ideology of the U.S. Supreme Court on differences in the success rates of direct parties before the Court. Simply, we seek to explain why some categories of litigants win more frequently than others when appearing before the Court. Previous explanations have attributed differential success rates in lower federal courts to, inter alia, disparities between litigants of different status in judicial experience and resources. We argue, however, that differential success rates in Supreme Court decisions have more to do with the ideological composition of the Court and the Court's receptivity to the different types of legal claims made by litigants of different status.

Previous research indicates that the status of litigants before American courts has substantial influence on judicial outcomes. Higher-status parties enjoy significant advantages in appellate courts and usually win. This has been demonstrated in the U.S. courts of appeals (Sheehan and Songer 1989) and, to a lesser degree, in state supreme courts (Wheeler et al. 1987). Curiously, the impact of litigant status on U.S. Supreme Court decisions has not been widely studied.

Among other arguments frequently advanced to explain status differentials in litigant success rates are that higher-status litigants typically possess superior resources or greater litigation experience (or both). Galanter (1974), for example, cites the advantage that corporations and especially governments enjoy as "repeat players" in comparison to less-experienced single-shot litigants. Repeat players are presumably better able to "play for the rules" in the legal process. They can settle cases likely to be lost in the courts and appeal cases they have the best chance of winning—and thus maximize their success rates.

In contrast, Sheehan and Songer (1989) stress the importance of resource differentials. They observe that even among single-shot litigants, the poor lose more frequently than wealthier individuals. Litigation, of course, is expensive. Litigants with superior resources can arguably retain better legal counsel, undertake more extensive research, and otherwise invest more in case preparation.

Another possibility, largely neglected in the literature, is that status differences in success rates reflect not only the relative resources and experience of litigants but also the values, ideological preferences, and prejudices of the Court. Numerous studies (Rohde and Spaeth 1976; Segal and Cover 1989; Tate 1981) document the effects that the political values of judges have on judicial decisions. Although no one to our knowledge has linked these values to litigant status, the connection is easily made in theory. Litigant status is related to the types of cases brought before the Court and to the positions taken by the parties. Cases brought by minorities, the poor, and individuals against businesses or government frequently emphasize claims of individual rights and liberties. Thus, they are likely to appeal to liberal values and to enjoy greater success in courts with liberal majorities. Conversely, cases brought by businesses against governments more typically seek regulatory relief, appealing to conservative values.

Our contention is that ideology is likely to be especially salient in cases before the U.S. Supreme Court. One reason is that litigant resource and experience differentials are likely to diminish both in size and importance at higher levels of the judicial system. Litigants whose cases have survived the gauntlet of the appeals process have proven they possess both considerable stamina and substantial resources. They also have acquired significant experience in the process. Assuming that status differences bear as much on decisions to grant certiorari as on decisions on merit, cases between litigants with the largest resource and experience differentials are unlikely to make it onto the Court's docket in the first place. Moreover, a case raising questions important enough

TABLE 1

Net Advantage of Parties, 1953–88 (%)

TYPE OF PARTY[a]	OVERALL SUCCESS RATE (1)	SUCCESS RATE AS RESPONDENT (2)	APPELLANT'S OPPONENTS SUCCESS RATE (3)	NET ADVANTAGE[b] (4)
Poor individual	31.2	38.5	70.5	−32.0
Minorities	55.6	44.3	34.9	9.4
Individual	42.9	35.2	52.6	−17.4
Unions	53.9	39.0	36.4	2.6
Small business	42.7	29.2	46.9	−17.7
Business	44.3	36.0	47.9	−11.9
Corporations	38.6	29.3	48.6	−19.3
Local govt	45.8	43.1	49.4	−6.3
State govt	54.1	51.0	39.8	11.2
Federal govt	67.3	59.1	23.2	35.9

[a] In order of increasing status.
[b] Col. 2 − Col. 3.

to be granted certiorari is likely to have sufficient cachet to attract quality counsel willing to argue the case for the visibility and prestige the case might bring. Parties to such cases can also frequently supplement their resources with contributions from concerned individuals and (especially) interest groups.

A second reason for hypothesizing that ideology will have a greater impact on the success of direct parties before the Supreme Court is our belief that ideological considerations are likely to be more sharply defined in cases reaching the Supreme Court. Again, because the Court controls its docket, it can select cases where peripheral issues are minimized and issues of principle predominate. Although there are exceptions, lower-level courts generally have less docket control and confront more cases where philosophical issues are obscured by other considerations.

DATA AND MEASURES

Data on the differential success rates of direct parties before the Supreme Court are available from the U.S. Supreme Court data base. The data base includes all cases decided with opinion by the Supreme Court between 1953 and 1988. Parties to these cases are identified by the labels given them in the Court's judgment or opinion. The detailed listing of parties in the data have been reduced to 10 categories: poor individuals, minority group individuals, other individuals, unions, small businesses, corporations, other businesses, local governments, state governments and the federal government.[1]

Since the Supreme Court data base is constructed from the written opinions of the Court, it does not provide direct measures of the financial resources or previous litigation experiences of litigants. To circumvent this problem we have adopted a strategy suggested by Wheeler and his colleagues (1987), ranking the 10 categories of litigants from strongest to weakest based on estimates of their financial resources and likely experience as repeat players in litigation. Although using status as a proxy for resources and experience precludes an assessment of the independent effects of resources versus experience, it does permit assessment of their combined effects vis-à-vis ideology.

To measure the changing ideological predisposition of the Supreme Court across the 36 years encompassed by this study, we use the ideology scores developed by Segal and Cover (1989) for individual justices.[2] To create a composite measure of the ideology of the Court we sum Segal and Cover's individual ideology scores across the nine justices who comprise the Court in any year.

RESULTS

Table 1 reports the success rates of the 10 categories of litigants in cases decided by the Supreme Court during the years 1953–88. As expected, success rates vary widely across the several categories of litigants, ranging from 67% for the federal government to only 31% for poor individuals. However, the overall success rates of litigants can be misleading. Because the Supreme Court is well known for its tendency to reverse decisions from below,[3] litigants whose cases are accepted on appeal are more likely to win than those who appear as respondents. To compensate for this, Table 1 also reports the net advantage of each class of litigant. This measure controls for appellant status by focusing upon the success rate of litigants in cases in which they are respondents and subtracting the success rate of their opponents in cases in which the opponents are respondents.[4]

If the resources and experience hypotheses are correct, we would expect the net advantage of the 10 litigant groups to vary directly with their status rankings. In fact, the relationship is quite weak. The federal and state governments enjoy significant advantages over other litigants, but so do minorities.

Individuals, especially the poor, are severely disadvantaged before the Court; but so are corporations and small businesses.

The overall net advantage of a category of litigants can be misleading, because the nature of the cases in which they typically are involved may bring them into conflict disproportionately with stronger or weaker opponents. To control for this possibility, we calculated the net advantage of each category of litigants against each of the other nine categories of litigants in our analysis. The results reinforce our observations both of the extreme variation in success rates among parties and of the limited impact of status differences (resources and experience) on success.

Predictably, during the 36 years spanned by our analysis, the federal government enjoyed very large net advantages against all other parties. But the federal government is the only party that consistently dominated lower-status parties. In Table 2, the party assumed to have superior resources and experience had a net advantage in 20 of the 29 comparisons (69%) for which sufficient cases are available. However, in 5 of those 20 cases, the advantage of the stronger party was negligible (i.e., five percentage points or less). Thus, the party presumed to be stronger enjoyed a clear-cut advantage in only 15 of 29 comparisons—just about what would be expected by chance if we knew nothing about the relative status of competing parties.

A different perspective on the resources argument is provided by examining the success rates of litigants over time. If resources and experience are determining factors in Supreme Court decisions, we would expect success rates to be relatively stable over time, since the relative resources and judicial experience of parties probably have not changed much over the years. In contrast, the ideological composition of the Court has changed dramatically. Thus, if ideology is a determining factor we would expect the success rates of different status litigants to vary significantly.

Table 3 displays the success rates for various combinations of litigants[5] across three periods corresponding roughly with the Warren Court (1953–70), the early Burger Court (1971–80) and the later Burger and Rehnquist Courts (1981–88).[6] Consistent with the ideology hypothesis and contrary to the resource hypothesis, the relative success rates of parties before the Supreme Court vary considerably. Predictably, individual litigants have suffered most from the growing conservatism of the Court, their success rates falling in relation to all other categories of litigants.[7] The pattern among other pairs of litigants is more complex. Businesses have fared better against the federal government but worse against state and local governments. State governments fared better against the federal government during the early Burger years but much worse during the Rehnquist years. Although the reason for this is not immediately apparent, an explanation is offered in a subsequent section.

TABLE 2

Net Advantage for Different Combinations of Parties, 1953–1988

COMBINATIONS OF PARTIES	NET ADVANTAGE BY %
Poor individuals vs.	
Federal government	Federal government, 52.4
State government	Poor individuals, 1.8
Minorities vs.	
Federal government	Federal government, 24.6
State government	Minorities, 12.2
Local government	Minorities, 24.7
Businesses (incl. corp. & sm.)	Minorities, 15.7
Individuals vs.	
Federal government	Federal government, 33.0
State government	State government, 17.6
Local government	Local government, 5.4
Businesses	Businesses, 2.1
Corporations	Individuals, 17.1
Unions	Unions, 10.6
Unions vs.	
Federal government	Federal government, 22.9
Businesses	Unions, 30.7
Corporations	Corporations, 16.7
Small businesses vs.	
Federal government	Federal government, 21.3
State government	State government, 23.1
Businesses	Businesses, 50.0
Corporations	Corporations, 5.7
Businesses vs.	
Federal government	Federal government, 43.8
State government	State government, .9
Local government	Local government, 5.7
Corporations	Businesses, 20.4
Corporations vs.	
Federal government	Federal government, 50.1
State government	State government, 28.6
Local government	Corporations, 32.1
Local government vs.	
Federal government	Federal government, 16.2
State government	Local government, 15.0
State government vs.	
Federal government	Federal government, 30.3

MULTIVARIATE MODELS

So far, examination of the resource and ideology hypotheses has been limited to comparisons of success rates across different combinations of litigants over time. Although illuminating broad patterns, these comparisons do not provide systematic estimates of the relative effects of either status or ideology. To obtain such estimates, we report logistic regressions in Table 4 for all Supreme Court decisions between 1953 and 1988 that involve some combination of our four aggregate litigant groups (i.e., individuals, businesses, state and local governments, and

TABLE 3

Success of Party Combinations over Time, 1953–88 (%)

TYPE OF PARTY	1953–70	1971–80	1981–88
Individual[a] vs.			
Business[b]	75.3 (174)	48.6 (138)	42.6 (129)
State and local	65.6 (518)	40.5 (870)	32.8 (585)
Federal	52.5 (503)	28.0 (414)	16.6 (259)
Business vs.			
State and local	58.7 (179)	42.6 (209)	47.5 (139)
Federal	25.2 (361)	27.6 (243)	31.5 (111)
State and local vs. federal	37.1 (35)	46.8 (47)	22.6 (31)

Note: Percentages are of cases won by first party listed. Number of cases in which the two parties opposed each other are in parentheses.
[a]Poor individuals, minorities, and other individuals.
[b]Small businesses, corporations and other businesses.

the federal government). Logistic regression is used since the dependent variable for each case is coded 1 if the litigant group won and 0 if it lost. Predictor variables in the four models include the status differential of the litigants (which serves as a proxy for differential resources and litigant experience)[8] and the aggregate ideological composition of the Court described previously. As a control variable, we also coded whether the litigant group in question was the appellant or respondent in the case.

Confirming the well-established tendency of the Supreme Court to reverse decisions from below, the models in Table 4 clearly indicate that all four classes of litigants have greater probabilities of success when they are appellants. The relationship is statistically significant and strong in all of the models and holds irrespective of the ideological composition of the Court or the relative resources of the litigants.

More important from our perspective, however, is the evidence of the relative effects of litigant resources and the ideology of the Court on the success of litigants. The evidence regarding the former is weak at best. In the business model and state and local government model, resource differentials appear to contribute significantly to the outcome of the case, although the size of the effect is quite modest. Resources, however, are not significant predictors of decisions in cases involving individuals or the federal government.

Further weakening the resource argument is the evidence that the effects of resources in the business model and state and local model stem almost entirely from the tendency of these parties to lose consistently to the federal government. In analyses in which cases involving the federal government are removed (not shown), resources cease to be statistically significant in the business model and are reduced to borderline significance in the state and local government model.

In contrast to resources and experience, the ideology of the Court has had considerable impact on the success rates of litigants of different status. This is most clearly seen for individual litigants who have enjoyed much greater success before the Court during more liberal periods regardless of the resources of opposing litigants or of their status as appellant or respondent.

Predictably, the ideology of the Court also has strong and significant effects on the success of state and local governments. Given that conservatism traditionally accords priority to the claims of order and community over the rights of individuals, it is not surprising that the success of state and local governments before the Court has been related inversely to the liberalism of the Court.

As we have seen, the federal government wins the great majority of its cases against all comers regardless of the relative resources of opposing parties or of the ideology of the Court. Nevertheless, the advantage enjoyed by the federal government in all situations increases significantly as conservatives gain in strength on the Court, although it increases at only about half the rate that the advantage enjoyed by state and local governments does.

TABLE 4

Logit Estimates for Models of Party Success

INDEPENDENT VARIABLES	INDIVIDUAL	BUSINESS	STATE AND LOCAL	FEDERAL
Appellant	.724*	.903*	.222*	.897*
	(.079)	(.104)	(.087)	(.102)
Ideology	.155*	−.018	−.140*	−.079*
	(.013)	(.032)	(.014)	(.016)
Resources	−.094	.231*	.257*	−.072
	(.062)	(.043)	(.058)	(.083)
Intercept	−.843	−.718	−.228	.653
Error reduction (%)	28	1	17	2

Note: Success = 1. Numbers in parentheses equal standard error.
*$p \leq .01$.

TABLE 5

Time Series Models of the Effects of Ideology on Direct Party Success

	INDIVIDUAL VS.			BUSINESS VS.		STATE VS.
	BUSINESS	STATE	FEDERAL	STATE	FEDERAL	FEDERAL
Ideology	2.9 (1.31)	3.7 (.973)	3.1 (.837)	1.1 (.611)	−1.3 (.393)	2.8 (2.00)
Noise model[a]	MA(3) −.39 (.156)	MA(3) −.29 (.145)	MA(1) −.34 (.094) MA(5) −.76 (.091)	—	MA(3) .39 (.162)	—
RMS[b]	289	193	80	289	100	1,239
ΔRMS[c]	14.1	18.5	35.4	18.8	25.9	2.3
Q[d]	14.0	7.1	6.8	5.4	5.8	12.0

Note: Models estimated with BMDP2T. Standard errors for parameter estimates in parentheses. Estimates more than twice as large as their standard errors are significant at the .05 level.

[a] Moving Average specification for noise; the order of the Moving Average process is in parentheses.

[b] RMS is the mean of the residual sum of squares for the model; it measures the model's "goodness-of-fit." Lower values indicate a better fit.

[c] Percentage improvement (i.e. reduction) of the full model compared to the noise model only.

[d] The Ljung-Box Q tests of autocorrelation in the model's residuals; with 12 degrees of freedom, none of the Q estimates are statistically significant at the .05 level.

A TIME SERIES PERSPECTIVE

The evidence so far suggests that the ideological composition of the Supreme Court has a much greater impact on the success of litigants than the resources the litigants possess. To better assess the aggregate effects of the changing ideological character of the Court on litigant success over the past three-and-a-half decades embraced by this analysis, we estimated a series of transfer function models using yearly success rates for each of the six pairs of litigants as dependent variables and the yearly index of the ideological composition of the Court as the independent variable. The transfer function models were estimated using standard Box–Jenkins methods (Box and Jenkins 1976). These enable us to measure the effects that ideology has had on the changing fortunes of the different categories of litigant over the 36-year period, while controlling for the well-known problems associated with time series (Granger and Newbold 1986).[9]

The transfer function models are reported in Table 5.[10] Since the ideology of each justice is measured on a scale of 1 (*most liberal*) to −1 (*most conservative*), the impact coefficients represent, in effect, the average increase in the success rate of one party over another resulting from the replacement of a relatively moderate member of the Court by a liberal member or a conservative member by a moderate one. For example, the coefficient for the impact of ideology on the success rate of individuals involved in litigation against state governments indicates that each unit-increase in the conservatism of the Court has reduced the success rate of individuals against states by slightly less that four percentage points. Although this impact may appear small, its effect over time can be substantial. Thus, from the high-water mark of liberalism on the Court in 1969 (*ideology* = 6.50) to the high tide of conservatism in 1988 (*ideology* = −2.02), the changing ideological composition of the Court was associated with a cumulative reduction in the success rate of individuals of almost 32 percentage points.

The pattern of effects in these models is very similar to that reported in the individual analyses. New is the evidence of the cumulative magnitude of the effect of the changing ideology of the Court. The impact of ideology on the success rates of individuals is especially dramatic. The increasing conservatism in the Court since the Warren years has been associated with a reduction in the success rates of individuals against business by 25 percentage points, against states by 32 percentage points, and against the federal government by 26 percentage points. As suggested in the analysis of individual cases, the conservative tide has hurt businesses relative to state and local governments. However, the cumulative loss, although statistically significant, has been less than 10 percentage points and has been compensated by an 11% increase in business successes against the federal government.

The one model in which ideology is not statistically significant involves cases where the federal government opposes local or state governments. Contrary to our expectation that the increasing conservatism on the Court should benefit states in their struggles against the federal government, the direction of the impact coefficient indicates that states have won less frequently against the federal government as the Court has grown more conservative. Although not statistically significant, the size of the coefficient is substantial. One possible explanation for this is simply that the number of cases involving the federal, state, and local governments as opposing litigants is relatively small. Since only slightly more than one hundred such cases have been decided over the

36-year-period, the success rates for states are highly volatile—especially in the early years, where the number of cases is especially small. Although this would account for the lack of statistical significance, it does not explain the large, contrary-to-theory impact coefficient.

Another possibility is that the impact of ideology on the success of the federal government may depend on the ideological or partisan complexion of the federal government as much as that of the Court. The increasing conservatism of the Supreme Court has not occurred in a vacuum. Rather, it is closely linked to the control of the executive branch of the federal government by conservative Republican administrations. Similarly, the increase in the liberalism of the Court in the 1960s was associated with the domination of that decade by liberal Democratic administrations. Although the ideological complexion of individual state and local governments also changes, the large number of states and localities and the cross-cutting nature of change mean that the aggregate ideology of states and localities is much stabler over time than the ideology of the federal government. Given this pattern, it is possible that the ideological orientation of the federal government as a litigant before the Court is linked to the ideology or partisan orientation of the administration and, therefore, that the success of the federal government versus states is a product *jointly* of the ideology of the Court and of the federal government. In other words, we hypothesize that Democratic administrations will win more frequently against state and local governments as the liberalism of the Court increases but that Republican administrations will win more frequently against states and localities as the Court becomes more conservative.

To test this possibility, we created a partisan-controlled measure of the success of state and local governments versus the federal government and estimated a new transfer function model of the impact of ideology. The results are encouraging. The impact coefficient is strong, negative, and statistically significant, indicating (as predicted) that the success of state and local governments against democratic administrations has declined as the Court has become more liberal and that state successes against Republican administrations have declined as the Court has become more conservative. Although the reduction in the residual mean square produced by this model still is quite small (about 5%), it is double the reduction achieved in the model without partisan controls.

Finally, the pattern suggested in these data help to explain the observation in Table 3 that the success of state and local governments against the federal government increased in the 1970s before declining in the 1980s. During the early 1970s, Republican administrations faced a still largely liberal Court where, as at the end of the decade, a Democratic administration faced a Court well on its way to becoming conservative. The result was an ideological and partisan environment much more conducive to the interests of states and localities than the 1980s with its conservative Court and Republican administration or the 1960s with a liberal Court and Democratic administrations.

CONCLUSION

Examination of the success of direct parties before the Supreme Court suggests that litigant resources and experience are considerably less important than in other appellate courts. The contrast between the Supreme Court and the U.S. courts of appeals is especially dramatic. Our analyses confirm the existence of marked differences in the success rates of different categories of litigants before the Supreme Court. However, these differences are not related consistently to litigant status and vary substantially over time in ways suggesting that these differences have little to do with litigant resources or judicial experience and much to do with the changing ideological composition of the Supreme Court.

Moreover, what little evidence there is for the importance of resources and experience stems almost entirely from the observed dominance of the federal government against all other categories of litigants. Although the federal government's consistent success before the Court may be a consequence partly of its superior resources and experience as a repeat player, it also may be due to a variety of other, non-resource-related factors. Among the more important of these, we speculate, is the long-standing *substantial evidence doctrine*, which encourages judicial deference to the federal government. In addition, our analyses provide evidence of what Dahl (1957) contends is a long-term political dynamic that tends to produce an ideological and partisan "alliance" between members of the Court and presidential administrations. This dynamic is most clearly evident in the increasingly conservative tenor of presidential Court appointments since 1980 and the Court's increasing support for the positions of conservative, Republican administrations during the same period.

Of course, this research can be faulted in a variety of ways. For example, the use of litigants' status as a proxy for their resources and judicial experience introduces considerable measurement error into the analysis, as does the identification of the ideological interests of litigants based on their group identities. As we have noted, even poor defendants with interesting cases can attract skilled and experienced counsel and the financial support of powerful interest groups. Were more direct measures of litigant resources available, we might observe smaller resource differences between litigants and these small differences might have greater effects on judicial outcomes.[11]

Similarly, the diversity of ideological points of view within categories of litigants means that the effects of ideology are almost certainly underestimated. For example, the weak effect that ideology appears to have on the success of state and local governments versus the federal government may result because state and local governments frequently press claims (e.g., that their environmental regulations not be overridden by more conservative federal standards) that are more liberal than our underlying assumption recognizes.

Clearly, direct measures of litigant resources and ideological claims would be desirable. Unfortunately they are unavailable. Moreover, such data could not reasonably be obtained for a set of cases as extensive as the Supreme Court data base without a commitment of time and money equal, perhaps, to those already invested in that project.

Nevertheless, although limited by the availability of data and the need to rely on proxies as measures for key concepts, we have provided the first systematic analysis of the success rates of different categories of litigants appearing before the Supreme Court. In addition to demonstrating that success rates of litigants vary widely both across different types of litigants and over time, the analysis is at least suggestive as to the causes of these differences. Theory suggests—and our data support—the argument that litigant success before the Supreme Court depends substantially on the ideological composition of the Court but little, if at all, on the resources and prior judicial experience of the litigants.

Notes

We wish to thank Harold Spaeth for providing us with a preliminary copy of the U.S. Supreme Court data base, phase I.

1. The category of poor individuals includes only parties who were clearly identified in the Court opinion as *indigent*. The individual category is obviously underinclusive, since it excludes individuals who are poor or members of groups usually considered to be protected minorities. It also excludes individuals who appear before the court in their professional capacity. Corporations are defined simply as very large business entities (e.g., airlines, railroads, banks, insurance companies, and oil companies). Small businesses are those which are more likely to have an individual owner–proprietor, including bookstores, realtors, restaurants, and theaters. The residual business category includes businesses in between the other two or whose sizes are ambiguous. A number of parties were not encompassed in any of these categories, including professional associations such as the American Medical Association, churches, clubs, environmental groups, public interest organizations, and nonprofit groups. Since the resource levels or ideological orientations for most of these groups were ambiguous and the number of cases involving a number of these groups were quite small, we have excluded them from the analysis.

2. The construction of these scores and their underlying assumptions and limitations are discussed in detail by Segal and Cover (1989). Simply, Segal and Cover assign each justice an ideological score between −1 (*most conservative*) and 1 (*most liberal*) based on content analyses of newspaper editorial commentaries of the justices' judicial philosophies at the time of their appointments to the Court. The measure was constructed by, in essence, computing the percentage of paragraphs in the editorials of four newspapers that were coded *liberal, conservative,* or *moderate* by three separate coders: "Liberal statements include (but are not limited to) those ascribing support for the rights of defendants in criminal cases, women and minorities in equality cases, and the individual against the government in privacy and first amendment cases. Conservative statements are those with the opposite direction" (p. 559). The resulting construction reflects the perceived ideology of new justices relative to prevailing values at the time. There were a few early justices who were not included in the Segal and Cover study. We estimated a score for these justices from the scale analyses reported in Rhode and Spaeth 1976 and Schubert 1965. Based on their relative scale scores, the justices were placed within the Segal and Cover scale. Most of these justices were involved in a small number of cases. The two who were involved in a large number of cases were Black and Douglas, both of whom it was very easy to classify as liberals.

3. During the period under study, the Court's reversal rate was 67%.

4. This measure reverses the logic advanced by Wheeler and his colleagues in measuring the effects of the opposite tendency of state courts to *affirm* cases from below (1987, 418).

5. Because the number of cases for several categories of litigants becomes very small when disaggregated over time, we collapsed the 10 categories of litigants used in other analyses into four categories: individuals, businesses, state and local governments, and the federal government. Since unions do not fit naturally into any of these categories and were involved in relatively few cases, we dropped them from the remainder of the analysis.

6. These periods were chosen primarily on the basis of political factors surrounding the appointment of new justices and the effect of these appointments on decisional trends. The 1953–70 period corresponds with the appointment of Chief Justice Warren up to the appointment of Justice Blackmun. After the appointment of Blackmun in the early Burger Court, we see a significant change in the number of liberal decisions emanating from the Court. During the period 1981–88, we see an even greater drop in the number of liberal decisions delivered by the Court. These three periods allow us to obtain an indication of the effect of changes in the Court's composition and ideological direction, on the success of different types of parties.

7. It could be argued that the individual success rates are the result of the low success rates that criminal defendants generally have in appellate courts. To control for this possibility, the success rates of individuals were analyzed for criminal and noncriminal cases. Utilizing the same court periods, the success of individuals in criminal cases were 58.1% (Court 1), 31.8% (Court 2), and 25.1% (Court 3). In civil cases, individual success rates were 61.1%, 41.7%, and 30.1%, respectively. Thus, we conclude that the success of individuals has consistently declined across time in both criminal and noncriminal cases, and these data provide further evidence of the impact of ideological change on the fate of individual litigants.

8. The *status/resource* variable assumes a status continuum, along which *individuals* = 1, *business* = 2, *state and local governments* = 3, and *federal government* = 4. Thus, when a case involves the federal government versus individuals as litigants, the status/resource differential is 3 or −3, depending on whether the model is explaining federal success or individual success. Similarly, when an individual opposes a business, the business would be coded as having a status/resources advantage of 1 if the individual was coded as having a disadvantage of −1.

9. The use of Box–Jenkins procedures also makes it possible to test for Granger causality in the relationship between the Court's ideology and litigant success rates (Freeman 1983; Granger 1969). Strictly speaking, Granger causality implies that lagged values of X explain variance in current values of Y that cannot be explained by past values of Y. In this case, it means that past values of the Court's ideology explain current litigant success that cannot be explained by past values of litigant success. (E.g., the introduction of lagged values for ideology result in a significant improvement in the residual mean square.) In fact, the best-fitting model specifications for all litigant groups is a model in which the effects of ideology on success rates are unlagged. In every case, a model specified with a lagged ideology effect of one year was also

significant, albeit somewhat smaller. Although the specification of unlagged ideology effects weakens the case for Granger causality in these relationships, the significant improvement in the residual mean square registered in all of the models except the state-and-local-versus-federal-government one provides at least modest evidence of Granger causality.

10. The impact coefficients in these models measure the expected change in the success rates of an opposing pair of litigants produced by a one-unit change in the aggregate ideology of the Court. Several of the models have significant moving average components in the error term. These indicate the existence of short stochastic cycles. The statistics at the bottom of the table provide summary measures of the models' performance and indicate that all of the models perform acceptably (again, except for the federal-versus-state-and-local model). Ideology produces reductions in the residual mean square in each model of between 15% and 35%, and the Ljung–Box Q-statistic indicates that the residuals in all models are appropriately free of autocorrelation.

11. It should be noted, however, that our measure of litigant status/resources is the same used in previous studies where resources have been shown to be significantly related to judicial success rates (Sheehan and Songer 1989). Thus, the suggestion that resource differentials are smaller and less consistent among Supreme Court litigants and have smaller effects on litigant success is less a problem of measurement than confirmation of our initial hypothesis about the limited impact of resources on Supreme Court outcomes.

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CONTROVERSY

QUESTION WORDING AND MACROPARTISANSHIP

MacKuen, Erikson and Stimson used quarterly Gallup poll data to show in this Review *in 1989 that changing levels of macropartisanship, the two-party division of partisans, responded to presidential approval ratings and perceptions of the economy and predicted national election results. In a 1991* Review *research note Abramson and Ostrom argued that the NES and GSS questions more commonly used by scholars generated macropartisanship measures less sensitive to short term factors and less predictive of election outcomes. In this* Controversy, *Erikson and Stimson respond to the challenge and present new data from CBS News and* New York Times *telephone surveys to buttress their earlier analyses, arguing against substantial effects of the different question wordings. Abramson and Ostrom explain their continuing reservations.*

In a 1989 article (MacKuen, Erikson, and Stimson 1989), we presented the case for macropartisanship as a political indicator. As we defined the term, *macropartisanship* is simply the two-party division of party identifiers, as observed from quarterly readings of the Gallup Poll going back to World War II. We demonstrated major variation over time in the Gallup-based measure of party identification. Moreover, according to our statistical analysis, this movement in macropartisanship follows changing presidential approval and perceptions of the economy as monitored by the index of consumer sentiment. Based on our findings, the net balance of party identification fluctuates regularly in response to the political forces of the moment and predicts national election results (see also Weisberg and Smith 1991). If correct, our thesis has important implications for the understanding of partisanship in the United States.

Paul Abramson and Charles Ostrom (1991) challenge our conclusions about macropartisanship. Abramson and Ostrom concede most of our findings to be correct for the Gallup-based measure of macropartisanship. They contend, however, that our findings would be quite different had we used the party identification question asked by the Survey Research Center (SRC) of the University of Michigan instead of the Gallup version. Gallup has asked the question two slightly different forms.

> In politics, as of today, do you consider yourself a Republican, a Democrat, or an Independent?
>
> In politics today, do you consider yourself a Republican, Democrat, or Independent?

The SRC asks,

> Generally speaking, do you usually think of yourself as a Republican, a Democrat, an Independent, or what?

Echoing an earlier conjecture by Converse (1976), Abramson and Ostrom claim that the Gallup wording (*In politics, as of today*) invites short-term responses to transient events, whereas the SRC wording (*Generally speaking*) encourages stable responses anchored by long-term partisan attachments.

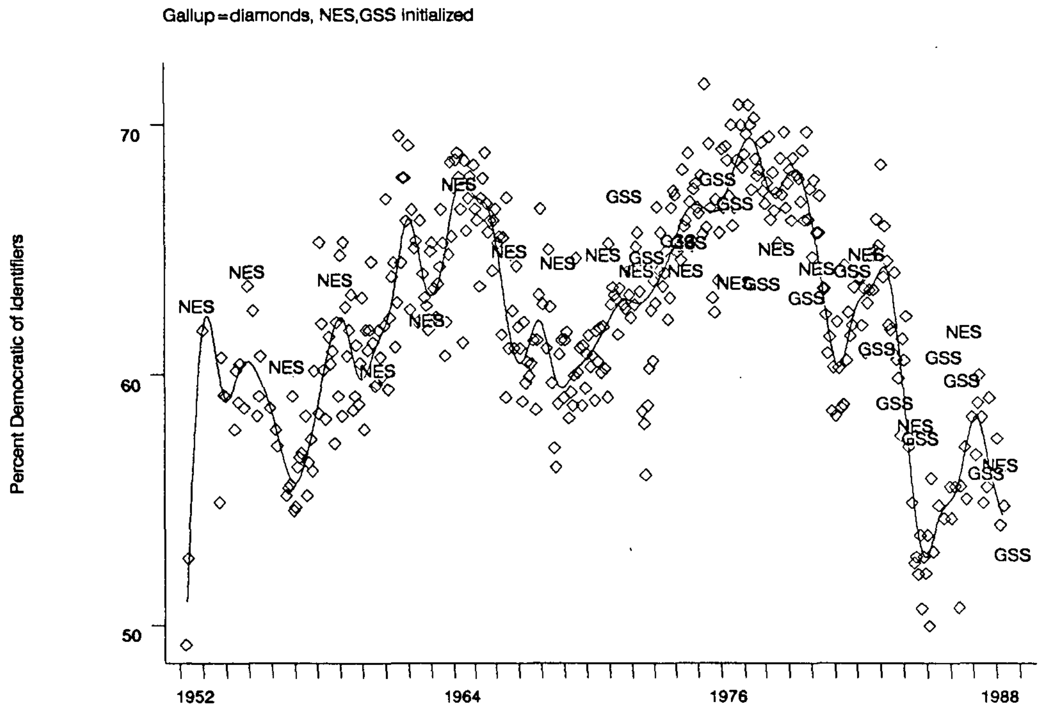
According to Abramson and Ostrom, most of our claims are "undermined" when the SRC question is used (p. 188). They say that we "exaggerate" the volatility of partisanship and "should have been more careful" in generalizing from the Gallup measure (pp. 190–91). For evidence, Abramson and Ostrom analyze the partisan division found in the 19 National Election Study (NES) surveys covering 1952–88, plus 16 General Social Surveys (GSS), each using the SRC question wording for party identification. Based on these limited time series, they claim that their preferred SRC version of macro party identification follows a more stable trajectory than the Gallup version. Moreover, they claim that their preferred SRC version is resistant to short-term influence from presidential approval or consumer sentiment. Abramson and Ostrom also question the electoral relevance of macropartisanship. They claim that our earlier paper overstates the degree to which even the Gallup version of macropartisanship can predict national election outcomes.

At stake is not just who got the story right but, potentially, the very meaning of party identification both for the individual and in the aggregate. More than ours, the Abramson-Ostrom position is compatible with the traditional orthodoxy regarding party identification, namely, that it rarely changes either for individuals or in the aggregate (e.g., Campbell et al. 1960; Miller 1991).[1] If Abramson and Ostrom are correct, true (NES) macro party identification moves sluggishly enough to sustain the traditional orthodoxy and our earlier report was a false alarm based on detection of major movement in the wrong variable.

Does so much turn on how different survey researchers frame the time horizon for their party identification question? We are thankful to Abramson and Ostrom for raising the matter of question wording. We certainly concede that question wording can be important in surveys. We also concede the likely presence of minor "house effects" affecting responses to Gallup, NES, and GSS party identification questions. But we also contend that Abramson and Ostrom's critique of our macropartisanship thesis misses the mark. As we will show, there is every reason to believe that our conclusions drawn from the Gallup time series would also hold for a lengthy time series of macropartisanship based on the SRC wording. Also (as we will show), routine movement in

FIGURE 1

Monthly Gallup Readings Overlayed with NES and GSS Readings of Macropartisanship



Note: Trend line is a computer-generated cubic spline for Gallup series.

macropartisanship does offer a clear signal of electoral change.

ARE THE GALLUP AND SURVEY RESEARCH CENTER PARTISAN SERIES DIFFERENT?

Abramson and Ostrom (1991) present a visual time series comparison of the NES and GSS party identification marginals with the Gallup marginals for roughly the same small number of quarters. Their NES and GSS readings (using the SRC wording) present a visibly and statistically flatter time series than the comparable Gallup quarterly readings. Figure 1 presents a comparison of all 35 NES and GSS readings for 1952–89 with the full set of 380 monthly readings (personal interview only) of the Gallup poll for the same 37-year period.[2] One cannot contest the fact that both series contain real movement. But consistent with Abramson and Ostrom's presentation, the NES and GSS readings display the least variance. The question is, Given the limited number of NES and GSS readings, do the NES and GSS "snapshots" of the macropartisanship series portray a phenomenon substantively different from the continuous Gallup series? On the one hand, virtually all NES and GSS observations are within the range of Gallup monthly observations for the same time period. And two key reference points—the 1964 Democratic peak and late 1980s Democratic nadir—are replicated nicely using NES and GSS measures. On the other hand, the sporadic NES and GSS series appears entirely to miss the "post-Watergate" Democratic surge of the mid-1970s captured so clearly by the Gallup measure. Are the two series really quite different, as Abramson and Ostrom argue? Or are the observed samples drawn from similar time series but with observable differences that are largely attributable to sampling error?

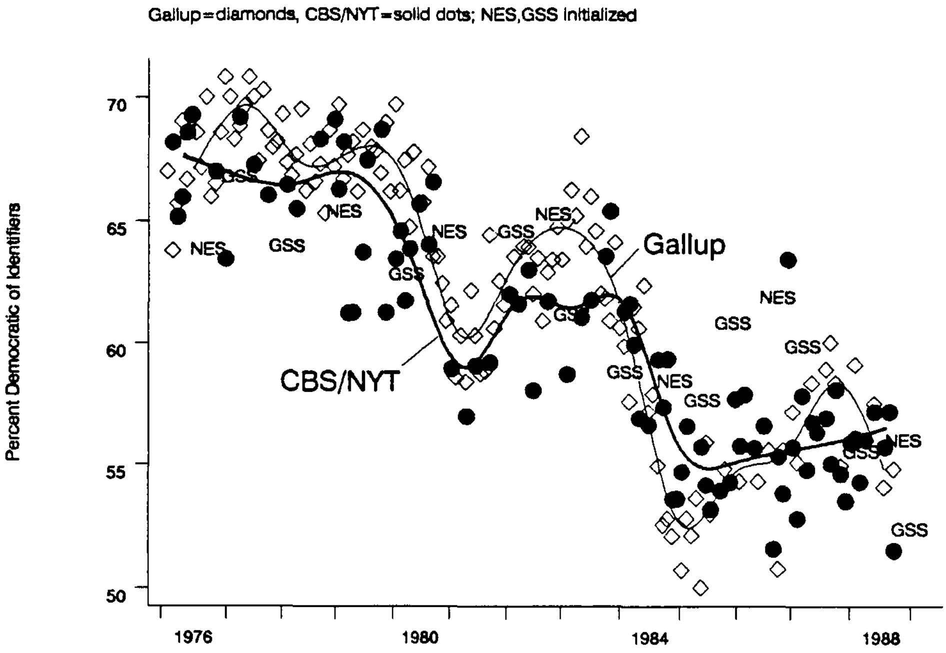
Happily, the NES and GSS are not the only surveys that measure party identification using the SRC question wording. Since 1976, CBS News and the *New York Times* (CBS–*Times*) have together conducted regular telephone surveys, using the SRC preface to the party identification question:

> Generally speaking, do you consider yourself a Republican, a Democrat, an Independent, or what?

Except for the innocuous substitution of *consider yourself* for *think of yourself as*, this is the SRC party identification question. Suppose we compare the CBS–*Times* party identification time series with the Gallup series for the same period. This comparison ought to decide whether the two-party identification questions produce substantially different marginals. Between 1976 and 1988, we have 133 monthly Gallup readings, with an average N of 2,052 partisan Dem-

FIGURE 2

Monthly Gallup and CBS–*Times* Readings of Macropartisanship, 1976–88



Note: Trend lines are computer-generated cubic splines.

ocrats or Republicans. For the same period, we have 88 CBS–*Times* readings, with an average N of 1,097 partisans.[3]

Figure 2 presents the CBS–*Times* versus Gallup comparisons, using monthly readings. (NES and GSS readings are also shown for comparison.) Figure 3 presents the same data in different format, as a comparison of two sets of collapsed "annual" surveys. Either way, there can be no doubt that the macro responses to the CBS–*Times* and Gallup party identification questions are virtually identical. At the beginning of the comparison in the later 1970s, the CBS–*Times* measure portrays the same post-Watergate Democratic superiority that the Gallup measure does. Pooled over all 1976–78 surveys, Gallup showed 68.0% of party identifiers to be Democrats (N = 81,195). For the same period, CBS–*Times* showed 67.2% of identifiers to be Democrats (N = 13,846). Also like the Gallup measure, the CBS–*Times* measure portrays the Republican resurgence of the 1980s. The Gallup and CBS–*Times* Democratic percentages of identifiers for 1986–88 are 56.4% (N = 31,626) and 55.6% (N = 28,777), respectively. The two measures also show similar bumps and wiggles over the 13-year span. Both show the two temporary Democratic surges (reflecting the "Reagan recession" and Iran–Contra).

The one difference is that the Gallup measure moves over a slightly greater range on the partisanship scale. This possible "house effect" may, indeed, be due to the difference in question wording highlighted by Abramson and Ostrom. But its magnitude is far too small to support an argument that the two measures measure inherently different things.[4]

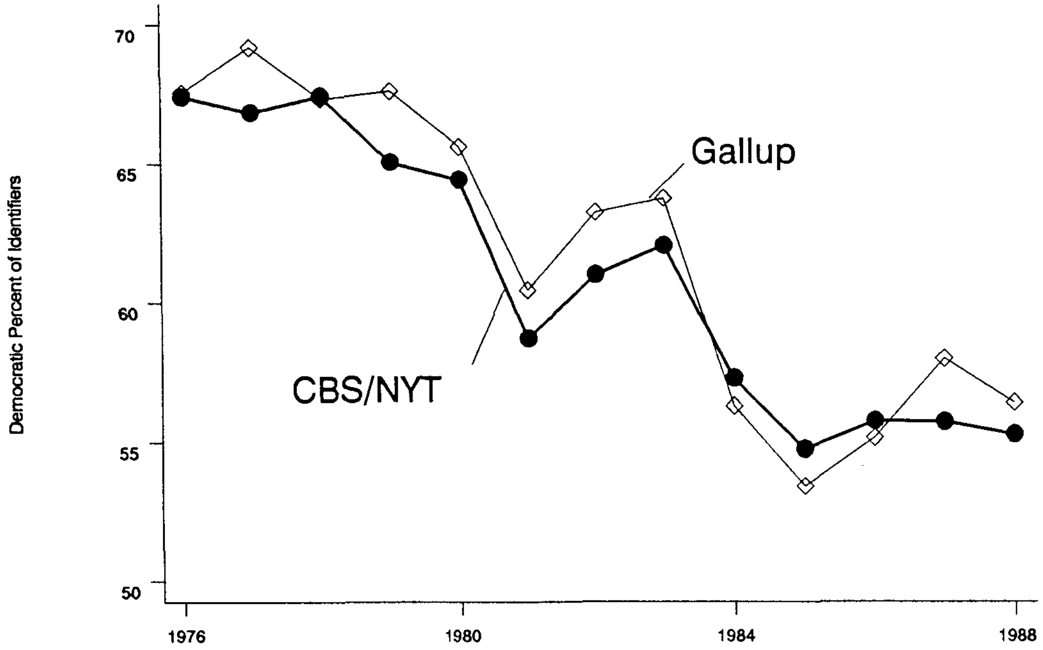
The Gallup versus CBS–*Times* comparison gives no support for any major difference in the party identification responses in respect to the time frame difference between the Gallup question (*In politics, as of today*) and the SRC question (*Generally speaking*). It is ironic, therefore, that the most obvious outliers among the observations of macropartisanship are found using the original NES and GSS "SRC" items themselves. Conceivably, an analyst might insist that the occasionally deviant marginals in the NES and GSS series (see, e.g., Figure 2) reflect the true measure of macropartisanship, while the more frequent Gallup readings and CBS–*Times* readings both offer false signals. But one can no longer use the rationale of question wording.

ACCOUNTING FOR MACROPARTISANSHIP

One of Abramson and Ostrom's claims is that unlike the Gallup measure, the SRC measure of party identification does not respond to presidential approval or consumer sentiment. Their evidence consists of a regression analysis predicting 18 NES readings of

FIGURE 3

Annual Gallup and CBS and NYT Readings of Macropartisanship, 1976–1988



SRC macropartisanship for 1954–88. In their analysis, the coefficients for consumer sentiment and presidential approval were not statistically significant and, in the case of approval, bore the wrong sign.

This sort of test is almost guaranteed to make the null hypothesis shine. The small number of cases (18), the gap of two years between observations, and the inevitable arbitrariness of their specification decisions all make any conclusions extremely shaky. The sparse data are not up to the task.[5] In any case, our demonstration with the CBS–*Times* series would seem to make the matter moot. Our 1989 analysis showed strong significant effects of approval and consumer sentiment, using the larger Gallup time series, with its intraadministration comparisons. There is every reason to believe that had an SRC-based series of similar length been available for analysis, it would have tracked the Gallup series closely. Therefore, it logically follows that an SRC-based equation would show something close to the same strong approval and sentiment effects as found for the Gallup series.

Still, some empirical support of this point may be appreciated. Accordingly, Table 1 presents the results of two sets of ordinary least squares equations predicting quarterly macropartisanship during the Reagan presidency. In one set, the dependent variable is Gallup readings of macropartisanship; in the other, it is CBS–*Times* readings of macropartisanship, using the SRC wording. Independent variables include Reagan approval and consumer sentiment (each lagged one quarter). To make the test difficult, we also include the lagged value of the macropartisanship measure from the previous quarter.

Levels of statistical significance and explained variance are stronger in the Gallup equation—expected, because the Gallup measure is more accurate due to the larger numbers of cases from Gallup surveys. In theory, the coefficients for approval and consumer sentiment should be similar in the two equations; and they are. The coefficients are higher in the Gallup equations, but the difference is slight. Collectively, the coefficients for approval and consumer sentiment are significant (.024) for the Gallup equation and on the edge of significance (.059) for the CBS–*Times* equation.

MACROPARTISANSHIP AND ELECTION OUTCOMES

Our 1989 analysis offered only a cursory analysis of the electoral relevance of macropartisanship. We reported that macropartisanship (in the third quarter of the election year) accounted for 38% of the variance in congressional seats and 23% of the variance in the congressional vote (1946–86). These are not strong statistical relationships. Abramson and Ostrom report even weaker ones when the time frame is shifted to 1952–88 and weaker ones still when the NES measure of macropartisanship is used. On the other

TABLE 1

Predicting Macropartisanship, 1981–88 (N = 26)

INDEPENDENT VARIABLES	GALLUP MACRO-PARTISANSHIP, *T*	CBS-*TIMES* MACRO-PARTISANSHIP, *T*
Gallup Macropartisanship, *t* − 1		
b	.53	—
st. err.	(.14)	
p	[.001]	
CBS/NYT Macropartisanship, *t* − 1		
b	—	.62
st. err.		(.14)
p		[.000]
Reagan approval, *t* − 1		
b	−.10	−.09
st. err.	(.07)	(.06)
p	[.21]	[.146]
Consumer sentiment, *t* − 1		
b	−.12	−.07
st. err.	(.04)	(.04)
p	[.011]	[.105]
Joint significance of Reagan approval, consumer sentiment	.024	.059
Adjusted R^2	.779	.654
Standard error of est.	1.84	1.84

Note: Macropartisanship is measured as percentage Democratic of identifiers. The cases are all Reagan quarters where both Gallup and CBS–*Times* measures of macropartisanship are measured for times *t* and *t* − 1. Six quarters are omitted, including quarter 1.

hand, by shifting the time frame from 1948–84 to 1952–88, Abramson and Ostrom boost the R-squareds for the presidential vote from .17 to .45 (Gallup's fourth-quarter macropartisanship) or .29 (NES).

Estimated as a crude bivariate time series relationship, the relationship between macropartisanship and election outcomes is not all that impressive. There are several contributing factors obscuring macropartisanship as an electoral indicator. First, single-survey estimates of macropartisanship contain too much error for precise readings. Second, the statistical relationships between macropartisanship and the vote (or seats) is not necessarily time-invariant. We would not necessarily expect, for instance, that any particular level of macropartisanship would have the same electoral result in both 1950 and 1990. Finally, a serious investigation should control for other causes of the vote, such as the effect of party control of the presidency in midterms.[6]

In investigating this problem, we learned that a revealing way to demonstrate the electoral connection is to relate electoral change to the observed change in macropartisanship between pairs of elections. The frequent (nearly monthly) readings of (Gallup) macropartisanship allow the measurement of partisan trends with considerable precision. For each biennial or quadrennial pair of elections, we measured the macropartisanship trend as the slope of the ordinary least squares equation predicting macropartisanship from time, in months (from January after election 1 to October prior to election 2). We index this variable as annual shift, so that a measure of 1.00, for instance, indicates a 1% Democratic gain in macropartisanship per year.[7]

Table 2 shows the results from a series of equations predicting the biennial change in the congressional (House) vote division from the biennial change in macropartisanship. The coefficients for all 22 interelection periods and for all 11 midterms (but not the periods from midterm to presidential year) are highly significant. The midterm result is particularly impressive. Two variables—the macropartisanship change and the party controlling the presidency—account for a huge 92% of the variance in the presidential-year-to-midterm vote change. The macropartisanship coefficient of .60 suggests that every 1% Democratic partisanship gain over the biennium is worth about a .3% gain in the congressional votes.

Furthermore, the macropartisanship shift over four years is related to the presidential vote change. With a coefficient over 4.0, each 1% change in macropartisanship over four years is associated with a vote change of greater than one percentage point in magnitude. The correlation, a modest .67, is illustrated in Figure 4. Note the one severe outlier, 1972. With this election omitted, the correlation between interelection change in macropartisanship and the interelection vote change soars to .89. (There is an excellent reason to consider dismissing the 1972 outlier. The Democratic 1968–72 gain in party identification was "artificially" generated by the introduction of the 18-year-old vote in 1971.)[8]

The ability of macropartisanship shifts to predict vote changes reinforces the importance of party identification as an electoral variable. If we were to find that no votes are affected when people collectively change their partisanship, it would be a major blow to the causal significance of party identification. Instead, when a small portion of the electorate shifts Democratic or shifts Republican, the change carries electoral consequences.

CONCLUSION

We hope that this response satisfies the doubts about macropartisanship raised by Abramson and Ostrom. Clearly, macropartisanship is important: it moves over a meaningful range, it responds to identifiable political forces, and it affects electoral outcomes. Further, the underlying phenomenon of macropartisanship does not depend on any particular question wording for its substance. What cannot be settled here is the important matter of reconciling macro movement of partisanship with micro theory about individual partisanship. That remains a challenge.

TABLE 2

Vote Change as a Function of Macropartisanship Change

	CONGRESSIONAL VOTE CHANGE[a]			PRESIDENTIAL VOTE CHANGE[b]	
INDEPENDENT VARIABLES	MIDTERMS	PRESIDENTIAL YEARS	ALL YEARS	ALL YEARS	EXCEPT 1972
Annualized 2-year change, macropartisanship					
b	.60	.67	.53	—	—
st. err.	(.22)	(.37)	(.21)		
p	[.024]	[.10]	[.022]		
Annualized 4-year change, macropartisanship					
b	—	—	—	4.34	5.45
st. err.				(1.61)	(.98)
p				[.024]	[.000]
Midterm party in power[c]					
b	−3.10	—	−3.17	—	—
st. err.	(.53)		(.81)		
p	[.000]		[.001]		
Constant	.28	1.12	.31	.89	2.91
Adjusted R^2	.920	.186	.633	.386	.767
Standard error of est.	1.29	3.10	2.39	6.90	4.07
Number of cases	11	11	22	11	10

[a] 1944–46 to 1986–88.
[b] 1944–48 to 1984–88.
[c] 1 = Democratic administration at midterm; −1 = Republican administration at midterm; 0 = presidential year.

POSTSCRIPT: MACROPARTISANSHIP IN 1989 AND BEYOND

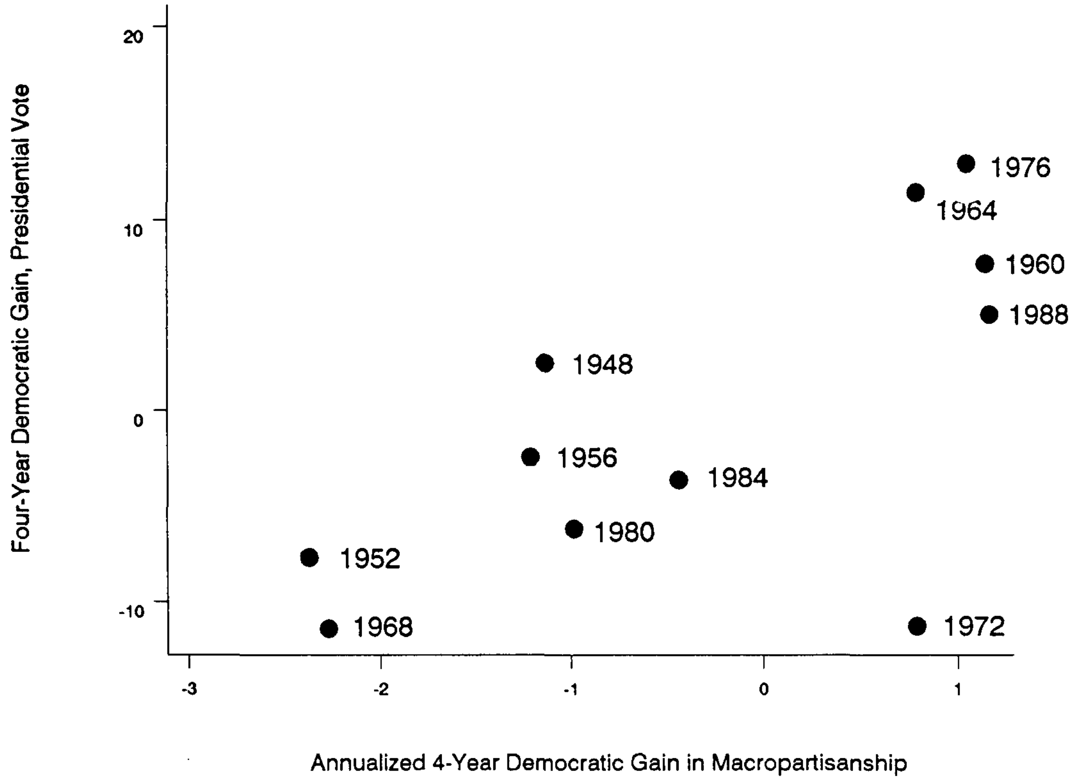
Measurement of macropartisanship beyond 1988 requires a change of data source because Gallup shifted almost entirely from surveys in person to the telephone variety between 1985 and 1988. We can, of course, measure post-1988 macropartisanship from Gallup's telephone interviews; but this requires some adjustment, since (for reasons that are not clear) Gallup's telephone surveys have always generated fewer Democrats than have surveys in person. For instance, whereas Gallup surveys in person and CBS–*Times* telephone surveys regularly showed similar levels of Democratic and Republican identification, Gallup's phone surveys persistently show more Republican identifiers than do CBS–*Times* surveys. Over the 1985–91 period, Gallup showed an average of 2.9% more Republicans among party identifiers than did the CBS–*Times* telephone series. Figure 5 shows the two telephone-based macropartisanship series from 1985 (when Gallup turned heavily to telephone series) through 1991. The figure shows the two series moving roughly in tandem, a few percentage points apart.

The post-1990 series offers a fresh challenge to our findings. During the Desert Storm operation in the Middle East, news reports suggested a surge to the Republican party, as if in gratitude for a successful war effort. Indeed, Gallup's 1991 surveys showed the first persistent Republican partisan pluralities since the 1940s. Was this Republican surge in some significant way an artifact of Gallup's question wording (*In politics today*) as opposed to the SRC and CBS–*Times* wording (*Generally speaking*)?

Figure 6 zooms in for a close-up of the monthly Gallup (telephone) and CBS–*Times* series from August 1990 to November 1991. Reflecting a continued decline in Democratic fortunes, the two series begin with only slim Democratic leads. Following a late 1990 pro-Democratic blip, both series show marked, if temporary, Democratic erosion for 1991. The war effect is most visible as the March 1991 pro-Republican spike in the Gallup series (a burst of about four points in the Republican direction), compared to a single-point movement in the CBS–*Times* series. Much of this Republican gain quickly dissipated.[9] Note also a minor Republican spike in August 1991, visible only in the Gallup series. This is the likely spillover from the failed Soviet coup. (CBS–*Times* polled immediately after the coup attempt; Gallup polled immediately after the restoration). Lest one imagine that the only movement is in the Gallup series, note that the CBS–*Times* series registers more dramatically the pro-Democratic shifts associated with the late 1990 budget fiasco and the late 1991 recession fever. The sudden appearance of persisting Republican pluralities in the Gallup data is not mainly due to lability in the question wording. Instead, it arises from the long-term decline in Democratic mac-

FIGURE 4

Presidential Vote Change by Macropartisanship Change, 1944–48 and 1984–88



ropartisanship (evident in both series) coupled with Gallup's shift to telephone interviewing.

Given the modest Republican surge of the Desert Storm period, the 1989–91 linear trend in Gallup's (telephone) series favored the Republicans at a rate of 1.4 points per year. Using the equations of Table 2, this trend in party identification translates into a projected Republican presidential vote gain of about five percentage points beyond the 54% Republican outcome of 1988. While this early forecast matches the conventional wisdom of 1991, later updates could turn with events of 1992, as macropartisanship responds to shifts in the economy and in presidential approval.

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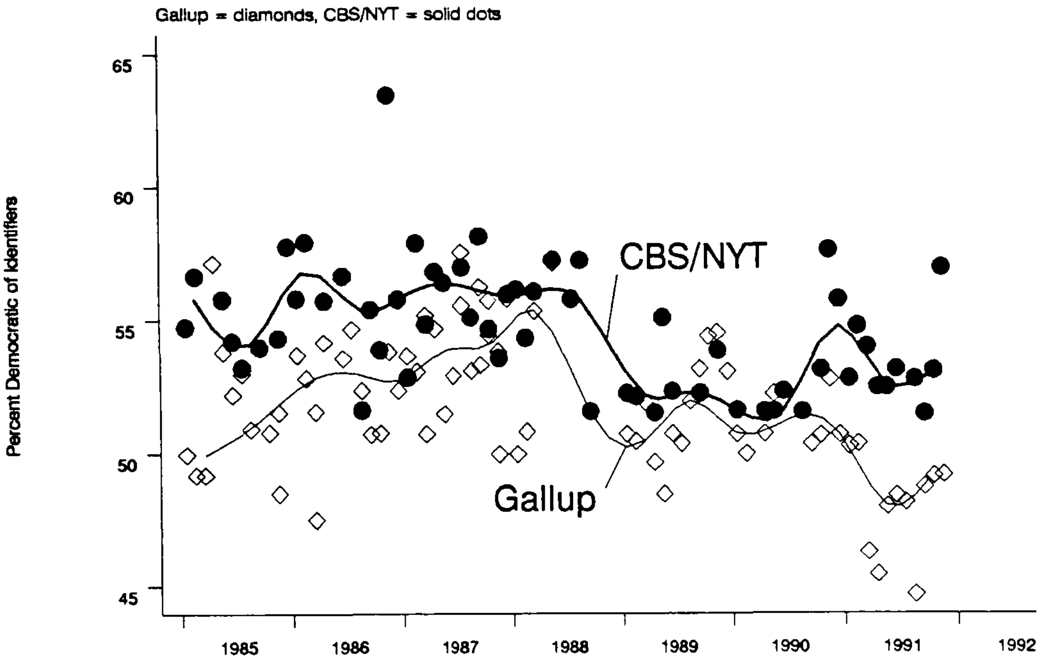
RESPONSE

In their original article on "macropartisanship" Michael MacKuen, Robert Erikson, and James Stimson (1989) made sweeping claims about the importance of their findings, arguing that there was more short-term variation in partisanship than would be expected based upon the traditional view of party identification.[10] They argued that their findings had important implications for the "standard view of party systems and realignment theory" (p. 1139). They suggested that salvaging the traditional view of party systems theory was futile: "As pre-Copernican astronomers preserved the Ptolemaic system, we may simply add medium-term partisan shifts to the longer-run cycles implicit in current party systems theory" (ibid.). Defending the traditional view of partisan change, they seemed to suggest, was as pointless as defending the geocentric view of the universe. At the same time, they argued that the implications of their findings for future theoretical work were unclear. "The mid-range dynamics" of partisan change, they argued, "are of tangible importance" (ibid.): "They yield partisan movements of realignment magnitude (though not realignment duration) that require neither miracles nor catastrophes but instead arise from the routine success and failure of ordinary politics. We argue for a quotidian, as well as a chiliastic, view of political change" (ibid.).

Despite these ambiguous theoretical claims, comparisons with astronomy are relevant in one basic respect. Just as advances in astronomy have depended upon accurate observation, advances in po-

FIGURE 5

Monthly Gallup (Telephone) and CBS–*Times* Readings of Macropartisanship, 1985–1991



Note: Trend lines are computer-generated cubic splines.

litical science also require proper measurement; and measurement lies at the heart of our controversy. In fact, scholars familiar with the research literature on partisan loyalties had good reason to suspect that MacKuen and his colleagues might have chosen a measure of partisan loyalties that enhanced short-term variation. Their analysis was based upon the Gallup party affiliation question, "In politics, as of today, do you consider yourself a Republican, a Democrat, or an Independent?" Although they reported that they were relying on Gallup data, they never reported the wording of the Gallup item or raised the possibility that their results might be affected by its wording. Yet the research literature had already suggested that the short-term focus of the Gallup item might lead to greater short-term variation in responses than the traditional SRC question widely used in the party identification literature (see Borrelli, Lockerbie, and Niemi 1987; Converse 1976). The Michigan question, by contrast, asks respondents, "Generally speaking, do you usually think of yourself as a Republican, a Democrat, an Independent, or what?"

Their justification for selecting the Gallup item was the frequency with which it has been asked. One of the main goals of our research note (Abramson and Ostrom, 1991) was to ascertain whether it was possible to compare the Gallup and the Michigan measure in a meaningful way. We reported that almost all of MacKuen, Erikson, and Stimson's findings were supported by our reanalyses when we used Gallup surveys. But we also demonstrated that similar results did not obtain when we used the party identification question employed biennially by the NES since 1952 and annually by the GSS since 1972. Both these surveys employ the standard Michigan question, which is designed to measure long-term attachments to the political parties. When the standard Michigan SRC question was employed, short-term variation in partisan loyalties was smaller, the total variation was reduced, and party loyalties were not significantly related to short-term outcomes in congressional elections. Moreover, our time series analyses demonstrated that short-term variation in the Michigan measure was not greatly affected by short-term evaluations of the economy and the president, whereas short-term evaluations explained substantial variation on the Gallup measure. Throughout our analyses, we demonstrated that the MacKuen, Erikson, and Stimson findings *could* be supported with Gallup data when biennial observations were employed. We therefore urged scholars to be cautious about generalizing from their findings to the research literature employing the Michigan SRC party identification question.

In their response, MacKuen and his colleagues question our actual findings in only a single instance.[11] But they argue that differences between the NES, GSS, and Gallup results do not result from differences in question wording. They base this conclusion mainly upon a comparison of 88 CBS–*Times* telephone polls conducted between 1976 and 1988 with 133 Gallup monthly readings based upon interviews in person. The CBS–*Times* polls, they argue, employ a preface very similar to the standard Michigan SRC question and allow scholars to compare the Gallup and Michigan SRC measure using a large number of observations.

In fact, in the course of preparing our note, we had considered using the CBS–*Times* data but decided that it might be questionable to compare the results of these telephone polls with the results of Gallup polls based upon interviews in person. To employ the CBS–*Times* telephone polls, one needs to weight the results to compensate for the tendency of telephone polls to underrepresent Democrats.[12] Although such weighting is feasible, we were a good deal less confident about using the CBS–*Times* series than we were in using the NES surveys conducted by the University of Michigan SRC and the GSS studies conducted by the University of Chicago National Opinion Research Center.

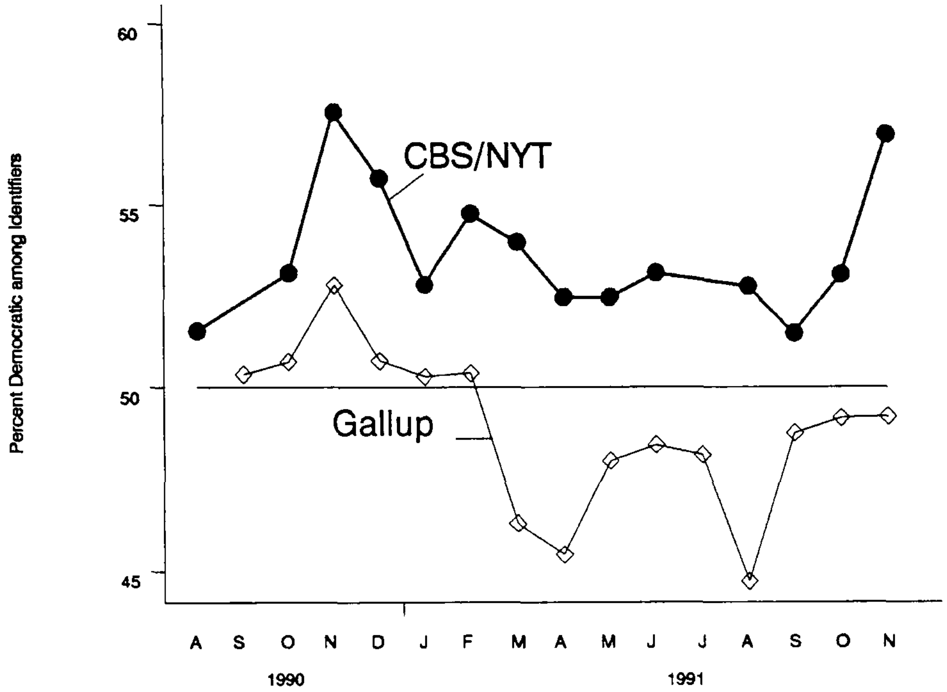
Although we remain skeptical about comparing trends based upon telephone polls with those based upon interviews in person, we are willing to suspend our skepticism if only because MacKuen, Erikson, and Stimson's analyses of the CBS–*Times* polls often support our conclusions. Short-term variation is greater with the Gallup surveys than with the CBS–*Times* surveys, and the total range of variation is greater. Moreover, between 1981 and 1988 changes in Gallup partisanship were significantly related to changes in consumer sentiment, whereas changes in partisanship as measured by the CBS–*Times* polls were not. Even so, Gallup and the CBS–*Times* results between 1976 and 1988 do not differ as much as results using the somewhat longer GSS, or the much longer NES, time spans.

MacKuen and his colleagues assert, "There is every reason to believe that had an SRC-based series of similar length been available for analysis, it would have tracked the Gallup series closely" (p. 478). In fact, there is good reason to question this claim. Our analyses demonstrate that Gallup and NES results correlate more closely during the 1970s and 1980s than they do when surveys from the 1950s and 1960s are included (see Abramson and Ostrom, 1991, Table 2). Differences are sharp if one compares the relationship between the NES trend and the Gallup trend for 1952–74 and 1976–88 (the years covered by the CBS–*Times* surveys). Between 1952 and 1974, our basic NES measure and the Gallup measure correlate at only .61; for the more recent period the correlation is .76. Most of our analyses were based upon a comparison of Gallup and NES results over the full 36-year period, and there is no way to test the claim that SRC and Gallup results would have been similar if a larger number of surveys employing the SRC measure had been available.[13]

However similar Gallup and CBS–*Times* results may have been between 1976 and 1988, there are major discrepancies between Gallup and CBS–*Times* results in more recent years. This may be seen in MacKuen, Erikson, and Stimson's postscript (pp. 480–81), which begins by comparing Gallup *telephone* polls conducted from 1985 through (November) 1991 with CBS–*Times telephone* polls during this period. They maintain that for this seven-year period the

FIGURE 6

Monthly Gallup (Telephone) and CBS–*Times* Readings of Macropartisanship, August 1990–November 1991



trend lines move "roughly in tandem" (p. 480). Although it is not practicable to assign exact values to the large number of data points plotted in their Figure 5, it is readily apparent that variation is greater with the Gallup data. Apart from a single outlier, the CBS–*Times* results go from a low of 51 to a high of 58, a 7-point range. The Gallup results go from 45 to 57.5, a 12.5-point range.

Most of their postscript, however, focuses on the period between August 1990 and November 1991; and the differences between the trends are substantial. Admittedly, the variation is only somewhat greater with the Gallup data (4.75 points according to our reading of their Figure 6) than with the CBS–*Times* surveys (3.82 points); and the total range is only slightly greater (8 vs. 6.5 points). But it would be difficult to argue that these trends move in tandem, since, according to our reading, they correlate at only a modest .51.

The most interesting difference between recent Gallup and the CBS–*Times* results occurs during and after the Gulf War, and several scholars have attributed this difference to question wording. In a recent article, Michael Kagay compared a series of Gallup telephone polls with telephone polls conducted by CBS–*Times*.[14] In interviews conducted between 14 and 24 February, 1991, when the air war in the gulf was underway but before the ground war began, Gallup found that 32% were Democrats and 32% were Republicans. The ground war ended on 27 February. In polls conducted between 28 February and 10 March, Gallup found that only 30% were Democrats, while 37% were Republicans. In polls conducted between 16 and 26 May, Democratic support crept up to 32% while the Republicans dipped to 33%. But CBS–*Times* polls, employing the Michigan SRC question and carried out over a similar period, never showed a Republican advantage. In four polls—conducted in late February, early March, early April, and early May—the Republicans remained constant at 29%. Democratic support dropped from 38% to 32% across these four polls. Gallup polls suggest that at least briefly, the Republicans had become the majority party among the electorate; the CBS–*Times* polls merely suggest that the Republicans came close to parity with the Democrats.

Kagay attributes differences between the Gallup results and the CBS–*Times* results to differences in question wording. He ends by quoting Everett Carll Ladd: "Some polls caught a Republican bounce, while others never showed it. A plausible reason is that some polling organizations' question wording invites people to think about their current predilection, while other polling organizations' wording asks people to think about their political heritage."[15]

Andrew Kohut (1991), who directs the *Times Mirror* polls, reaches similar conclusions. He notes that both the Gallup polls and the *Times Mirror* polls (which use the Gallup question) found Republican pluralities shortly after the Gulf War, whereas the CBS–*Times* and the ABC News–*Washington Post* polls (which use the Michigan question) did not.[16] Kohut argues that these differences are likely to result from differences in question wording:

> There is some evidence that while the small differences in the prefaces are not material during politically stable periods, they take on greater meaning when people are rethinking their partisanship. The preface to the Gallup and the Times Mirror question emphasizes *now*, while the preface to the other formulations is more likely to get people thinking about their traditional allegiances. (p. 22)

Moreover, comparing the annual results of Gallup interviews in person and CBS–*Times* telephone poll results between 1981 and 1990, Kohut argues that "the Gallup measure seems to change more in response to contemporary events than does the CBS/Times question" (ibid.).

Kohut concludes that "what appears clear is that the Gallup and Times Mirror question form magnifies the public's partisan reconsiderations, while the other format tends to anchor respondents somewhat more during periods of change" (1991, 22). "The Gallup approach," he continues, "gives a fuller indication of how much public reconsideration has occurred, while the CBS/Times format provides a more robust measure which may have more analytical utility—when, for example, it is such structural features of party support as social group composition that are to be analyzed" (ibid.).

In spite of our reservations about their conclusions, MacKuen, Erikson, and Stimson's response to our research note provides a valuable service by raising the possibility that question wording differences are not the cause of the substantial variation in partisanship they documented in their original article. Although it is possible that these recent differences between the Gallup and CBS–*Times* results—as well as the differences we found between Gallup, NES, and GSS results over a much longer period—do not result from question wording, we believe the preponderance of the evidence suggests that question wording does make a substantial difference.

A future research strategy to determine why the Gallup results differ from the NES and GSS results may require experimental studies employing both items in the same surveys to determine the extent to which responses are correlated at the individual level. There may well be individuals who consider themselves to be Republicans "as of today" but "generally speaking" usually think of themselves as Democrats. And there may be short-term Democrats who usually think of themselves as Republicans. Without individual-level tests, the question wording explanation cannot be definitively accepted or rejected. We think that it remains the most plausible explanation, however, for the very substantial differences between the Gallup and NES results that obtain when comparisons are made over the nearly four decades for which such comparisons are possible.[17] We would therefore continue to urge scholars to remain cautious about generalizing from the MacKuen, Erikson, and Stimson findings to the research literature based on the Michigan SRC party identification question.

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Notes

Abramson and Ostrom wish to thank Ada W. Finifter and Brian D. Silver for their suggestions.

1. Individual-level change and macro change are different phenomena. In an important contribution to the understanding of party identification, Green and Palmquist (1990) report impressive statistical evidence of only minimal movement of individual partisanship. This finding must be incorporated into any complete theory of party identification. Although Palmquist and Green portray a certain micro stability to party identification, this stability is not incompatible with our macro findings. By definition, their micro results must be compatible with the net aggregate change found in the NES panel studies from which they were drawn. Green and Palmquist's study reminds us that a small number of individual-level changes (due to what are sometimes called "period effects") can generate impressive-looking net movement on the macro scale of measurement.

2. All Gallup readings are obtained from the Roper Center. Where the sample number of cases was missing in a few surveys, we conservatively set it at one thousand partisans. National Election Study party identification is measured preelection in presidential years and postelection in midterms. We centered presidential-year NES readings in October and midterm NES readings in December of the election year, GSS readings in March.

3. CBS–*Times* readings were obtained as part of the Wright, Erikson, and McIver (1985) study of state politics. We exclude surveys of registered voters only. Given our interest in the national marginals, our inferences are based on the "weighted" rather than "raw" survey data. Since all CBS–*Times* surveys are telephone surveys, they are constant in survey method. Similarly, we include only Gallup surveys based on personal interviews, to maintain a constancy of method.

4. Whether the two measures are about the same can be subjected to statistical evidence, as well as mere "visual" tests. Consider the correlation between the Gallup and the CBS–*Times* (SRC wording) measurements of macropartisanship. For quarterly readings, this correlation is an impressive .917. This correlation is attenuated somewhat, however, by sampling variance in the two surveys. Let us conduct the technical exercise of estimating this sampling variance and adjusting the correlation estimate accordingly.

Assuming simple random sampling, we can estimate the sampling variance of a survey as the product of the two proportions, divided by the sample size. The reliability estimate for each measure of macropartisanship is the estimated ratio of true to total variance, or the ratio of the observed variance minus the mean sampling variance over the observed variance. For the set of 47 quarters with both Gallup and a CBS–*Times* readings of macropartisanship, the estimated reliabilities are .983 for Gallup and .944 for CBS–*Times*. Using these estimates to correct for reliability, the correlation between contemporaneous quarterly Gallup and CBS–*Times* measures of macropartisanship rises to .952. This high correlation is consistent with our contention that the two indices of party identification measure virtually the same thing. Consider, however, that the adjusted correlation falls short of perfection. The missing .048 we concede to differences in "house effects."

We can also examine the corrected correlations between macropartisanship measures in successive quarters. Corrected for reliability, the over-time correlations between macropartisanship at quarter *t* and quarter *t* + 1 are .965 for the Gallup measure and .945 for the CBS–*Times* measure. Corrected for reliability, the cross-correlations are .918 between the CBS–*Times* measure at time *t* and the Gallup measure at time *t* + 1 and .950 between the Gallup measure at time *t* and CBS–*Times* at *t* + 1. Thus, from the perspective of quarter-to-quarter change, macropartisanship is quite stable.

One difference between the two measures of macropartisanship using the two different question wordings is that the Gallup measure does have a larger range. Although this difference is consistent with Abramson and Ostrom's argument, it is slight. For the 47 quarters with joint readings of the Gallup and CBS–*Times* measures, the observed standard deviations are 5.72 for the Gallup measure and 4.98 for the CBS–*Times* measure. Corrected for reliability, the standard deviations fall slightly to 5.67 and 4.84. The ratio of the two standard deviations is .85. This is a modest difference in scale. The suggestion is that when the Gallup index moves 20 points, the CBS–*Times* index moves 17 points.

5. Part of Abramson and Ostrom's argument is that while consumer sentiment and presidential approval cannot predict NES partisanship with only 18 cases, the same 18 quarters are enough to predict Gallup macropartisanship from consumer sentiment and presidential approval. We have tried to replicate their first two equations (Abramson and Ostrom 1991, Table 4), using their reported specifications but were unsuccessful. We can report the reports of our replication using our own specifications. We measure macropartisanship for the party controlling the presidency and add a control for the party in power. This adds a degree of freedom but avoids the complex tangle of Abramson and Ostrom's standardization algorithm. (Standard errors are in parentheses.)

Independent Variable	Gallup	NES
Lagged partisanship	.52 (.23)	.12 (.36)
Consumer sentiment	.19 (.08)	.11 (.06)
Political approval	.10 (.10)	−.01 (.10)
Democratic president	11.08 (5.89)	23.30 (9.25)
R^2	.946	.966

Note that *political approval* represents a measure of *presidential approval* with the contribution of *consumer sentiment* removed. This variable is not significant in either equation. (Nor is "raw" presidential approval, when substituted.) Consumer sentiment is modestly significant in the Gallup equation but misses in the NES equation. The difference in the two consumer sentiment coefficients is trivial, however, considering their standard errors. Based on these two equations, one cannot make a serious case that consumer sentiment affects Gallup macropartisanship but not NES macropartisanship. The problem with making any kind of inference from these 18 cases lies with the limitations of the data.

6. In estimating the slope of macropartisanship on time for interelection intervals, we weighted the Gallup surveys by their number of respondents. Thus, our procedure was to pool the interelection set of Gallup partisans (as if it were a single survey) and regress their party identifications on time. We refrained from including postelection November and December readings in our estimates of partisan trends, because of a possible postelection macropartisanship bounce.

7. The presentation here does not exhaust ways of demonstrating the electoral importance of macropartisanship. Interested readers are referred to Stimson's treatment of predicting national election results from (among other variables) macropartisanship (1991, chap. 5).

8. One solution is to reestimate the 1969–72 measure of macropartisanship change with a dummy variable included for February 1971 (when Gallup began interviewing 18-year-olds) or later. The 18-year-old effect on macropartisanship is about three percentage points. With the revised estimate of partisan change for 1969–72, discounting the effect of the 18-year-old vote, macropartisanship change accounts for 62% of the presidential vote change.

9. Other polls using SRC's *Generally* preface (e.g., ABC–

Washington Post and NBC–*Wall Street Journal*) echoed the CBS–*Times* reading of a slim Democratic lead throughout the Gulf War, with no more than modest movement in the Republican direction. Meanwhile, the *Times Mirror* poll, which, like Gallup, asks respondents about their "politics today," probably showed the largest shift in party identification during the war. The *Times Mirror* readings shifted to Republican pluralities in January and March pollings; but unlike Gallup's, the *Times Mirror* readings bounced back to Democratic pluralities in May and July (Kohut 1991).

10. For the classic statement of the standard view of party identification, see Campbell et al. 1960. For a recent statement of this position, see Miller 1991.

11. MacKuen, Erikson, and Stimson maintain they could not replicate our findings that changes in the Gallup measure were related to changes in consumer sentiment and presidential approval when only 18 observations were employed. They complain that our procedures involve a "complex tangle" of a "standardization algorithm." We find their claim surprising, since we employed their standardization procedures for both consumer sentiment and political approval (1989, 1140–41, nn. 6, 13). Standardization does affect coefficients, but it does not affect t-ratios. When we compute the t-ratios for the Gallup equations in their response (p. 485, n. 5), we find that the t-ratios for both consumer sentiment (2.38) and political approval (1.00) are very close to the t-ratios in Abramson and Ostrom 1991, Table 4 (2.32 and 1.11, respectively). Moreover, both their results and ours reveal the same basic finding. Consumer sentiment is significantly related to Gallup partisanship but not to partisanship as measured by the NES.

12. In a recent interview in *Public Perspective*, Alec Gallup, cochairman of the Gallup Organization, acknowledges that the Gallup telephone polls regularly show a higher percentage of Republicans than Gallup polls based upon interviews in person. Moreover, he argues that these biases probably go beyond differences in socioeconomic status. He explicitly argues that telephone polls should not be compared with interviews in person. Although Gallup now employs both telephone polls and interviews in person to measure party affiliation, he maintains, "We feel that the two sets of data should be kept separate in charting trend lines" (quoted in "Measuring Things" 1991, 23.)

13. Part of MacKuen, Erikson, and Stimson's critique provides additional evidence that changes in Gallup partisanship are related to short-term electoral outcomes. Although we found somewhat weaker relationships between Gallup partisanship and congressional election outcomes than they reported, we were examining change during a somewhat different period. In all of our analyses (see Abramson and Ostrom 1991, Table 3), changes in Gallup partisanship were significantly related to changes in both congressional and presidential election outcomes. Since we never questioned MacKuen, Erikson, and Stimson's conclusion that Gallup partisanship is related to electoral outcomes, we will not discuss this portion of their critique.

14. Michael R. Kagay, "Rise in GOP Loyalty Now Ebbing, Poll Finds," *New York Times*, 2 June, 1991. For further details about these Gallup surveys, see Larry Hugick, "Post-War Surge in GOP Affiliation Already Slipping," Gallup Poll News Service, 10 April, 1991.

15. Quoted in Kagay, "Rise in GOP Loyalty Now Ebbing."

16. MacKuen, Erikson, and Stimson note that the *Times Mirror* polls probably showed the largest pro-Republican shift during the Gulf War but offer no explanation for their volatility.

17. With the addition of the 1990 NES survey, comparisons between Gallup and NES results are now possible over a 38-year period.

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NOTES FROM THE MANAGING EDITOR

Subfields, Manuscripts and Representation in the *APSR*

As is the lot of new managing editors, I have received a number of letters from scholars concerned that their subfield of political science is underrepresented in the pages of the *APSR*. I want to share my response to such concerns with all of our readers.

I recognize and emphasize that the official scholarly journal of the American Political Science Association has a responsibility to publish outstanding scholarship in all the subfields of political science. If you are concerned that work in your field is not appearing regularly in the *Review*, I urge you to help us. You can do so in three ways:

1. Encourage scholars in your field to submit their manuscripts to the *APSR* when they feel they have produced an outstanding piece of scholarship. We cannot publish what we do not receive. The assumption that *APSR* is unsympathetic to a particular subfield will become a self-fulfilling prophecy if the best work is sent elsewhere. As the various studies reported in *PS* by previous Managing Editors have repeatedly demonstrated, articles appear in the *APSR* roughly in proportion to the submissions from the subfield. It is not true that reviewers in various subfields exhibit strongly differential biases for or against their peers.
2. Encourage scholars in your field to follow the rules for submission, reprinted in each issue and discussed at more length in the *APSA Style Manual*, available from the Association. Follow the *APSA* format and style guidelines. Write a brief and clear abstract, which assists our evaluation process. Be particularly aware of our page limits. We can only publish about one manuscript from every ten submitted. Under these conditions we are unlikely to publish manuscripts that are substantially beyond our limit of 30 double-spaced pages. We do not even evaluate manuscripts whose total contents exceed 50 double-spaced pages, counting figures and tables, as well as double spaced notes, references and appendices. These limitations do create a bias in publication against work that cannot adequately be presented at article length. Most scholarly research, however, can be compressed or subdivided to fit journal constraints. No one likes to do this, but it can be done. Most often, tightening the presentation improves it.
3. The determination of quality is made primarily through the peer review process. Help us to understand your field and help us to find the appropriate reviewers for it. We are glad to be educated. We are always looking for skilled reviewers. Concerned about bias, we cannot follow the proposals of authors who send a selected list of referees with their manuscripts. But we are attempting to compile and check with members of our Editorial Board lists of reviewers appropriate for each subfield. Educate us before you submit a particular manuscript.

If your field does not publish frequently in the *APSR*, it might be especially useful for you to write me a brief letter, defining your field, outlining briefly its major concepts and theories, offering examples of outstanding recent research that can serve as benchmarks, and suggesting the names and addresses of peer reviewers. In the case of proposed peer reviewers, which is our most essential need, it is helpful to know about both their technical skills and their substantive interests. Having the best reviewers is essential for selecting the best manuscripts and for providing useful critiques and advice to authors whose manuscripts are not published.

I want to work with you to see that the *APSR* publishes the outstanding scholarly research in all fields of political science.

Transition to New Size and Press

We have now completed a second issue with the larger page size and the new press. Such a complex double transition involves overcoming many difficulties. I want to thank Cathy Rudder, Rob Hauck and, especially, Sheilah Mann of ASPA for their roles in supporting the transition and helping us work through the many details of the new arrangements. I also want to thank Assistant Editor Linda Lindenfelser for her untiring efforts and Tom MacDaniel of Byrd Press for his patient and cheerful assistance to the new editorial team.

Paper Error in March Issue

Due to a production error, the March 1992 issue of the *Review* was printed on paper with a glossy, rather than matte, finish. I sincerely regret the difficulty in reading the text printed on glossy paper.

FORTHCOMING IN SEPTEMBER, 1992

The following articles, research notes, and controversies have been tentatively scheduled for publication in the September, 1992, issue.

Jeffrey Berejikian. "Revolutionary Collective Action and the Agent-Structure Problem."

Bruce Bueno de Mesquita, Randolph M. Siverson, and Gary Woller. "War and the Fate of Regimes."

Alexander M. Hicks and Duane M. Swank. "Politics, Institutions, and Welfare Spending in Industrialized Democracies, 1960–1982."

John Huber. "The Role of Restrictive Legislative Institutions in France."

Michael B. MacKuen, Robert S. Erikson, James A. Stimson. "Peasants or Bankers? The American Electorate and the U.S. Economy."

Joshua Mitchell. "Protestant Thought and Republican Spirit: Luther's Peculiar Enchantment of the World."

Mark Alex Peterson. "The Presidency and Organized Interests: White House Patterns of Interest Group Liaison."

John T. Scott. "The Theodicy of the Second Discourse: The 'Pure State of Nature' and Rousseau's Political Thought."

Andrew Stark. "Corporate Electoral Activity, Constitutional Discourse, and Conceptions of the Individual."

Dana Villa. "Postmodernism and the Public Space."

Jan E. Leighley and Jonathan Nagler. "Socioeconomic Bias in Turnout 1964–1988: The Voters Remain the Same." A research note.

L. Marvin Overby, Beth Henschen, Michael Walsh and Julie Strauss. "Courting Constituents: An Analysis of the Senate Confirmation Vote of Justice Clarence Thomas." A research note.

Mark Schneider and Paul Teske. "Toward a Theory of the Political Entrepreneur: Evidence from Local Government." A research note.

Michael D. Ward and David R. Davis. "Sizing Up the Peace Dividend: Economic Growth and Military Spending in the United States, 1948–1996." A research note.

Richard D. Anderson; Margaret G. Herman and Charles Herman. "A Process Model vs. a Domestic Bargaining Model of Self-Frustrating Foreign Policy Decisions: Soviet Arms for Egypt in 1973." A controversy.

ERRATA

"The Political Manipulation of Macroeconomic Policy" by John T. Williams, September 1990, pp. 767–795. On page 790, column 1, sentence beginning on line 12, the reference to Tufte (1978) should be omitted.

In Bart Schultz's review (March, 1992, p. 215) of Chandran Kukathas and Philip Pettit's *Rawls: "A Theory of Justice" and Its Critics*, references to "The Theory of Justice" should read "A Theory of Justice."

In Edella Schlager's review (March, 1992, p. 231) of Donald Chisholm's book, the title given as *Structures in Multiorganizational Systems* should read *Coordination Without Hierarchy: Informal Structures in Multiorganizational Systems*.

BOOK REVIEW ESSAY

American Political Science Review Vol. 86, No. 2 June 1992

THE ROLE OF SEX AND GENDER IN COMPARATIVE POLITICAL ANALYSIS: INDIVIDUALS, INSTITUTIONS, AND REGIMES

BARBARA J. NELSON *University of Minnesota*

Engendering Democracy in Brazil: Women's Movements in Transition Politics. By Sonia E. Alvarez. Princeton: Princeton University Press, 1990. 304p. $45.00 cloth, $14.95 paper.

Women in Taiwan Politics: Overcoming Barriers to Women's Participation in a Modernizing Society. By Chou Bih-er, Cal Clark, and Janet Clark. Boulder: Lynne Rienner, 1990. 207p. $30.00.

Bringing Women In: Women's Issues in International Development Programs. By Nüket Kardam. Boulder: Lynne Rienner, 1991. 137p. $26.50.

Persistent Inequalities: Women and World Development. Edited by Irene Tinker. New York: Oxford University Press, 1990. 302p. $32.50 cloth, $15.95 paper.

Playing the State: Australian Feminist Interventions. Edited by Sophie Watson. New York: Verso, 1990. 243p. $59.95 cloth, $17.95 paper.

Women and Social Protest. Edited by Guida West and Rhoda Lois Blumberg. New York: Oxford University Press, 1990. 406p. $49.95 cloth, $18.95 paper.

In 1955 Maurice Duverger published *The Political Role of Women*, the first behavioralist cross-national comparison of women's electoral participation to use both election and survey data. Within the limits of the methodological individualism that shaped Duverger's research, his work set an early standard for the sensitive analysis of women's electoral activities. But Duverger's research was also severely hampered by its inability to theorize the relations between women and men and its silences about regime and economic factors. As such, his research foreshadowed the enduring limitations this approach bequeathed to the cross-national study of women's political participation. The strengths and weaknesses of Duverger's research are thus important points of departure for evaluating six recent books on women, gender, and the political economy.

Duverger analyzed women's patterns of voting, standing for election, serving in legislatures, and party and pressure group activity in Norway, France, the Federal Republic of Germany, and Yugoslavia in the period from the early campaigns to win female suffrage through the decade following World War II. In general, he found that across these four countries women voted somewhat less frequently than men (the difference narrowing the longer women had the franchise) and were slightly more conservative than men (findings sensitive to the sex and age composition of the population) and that married couples tended to vote similarly (whether men exercised "marital authority" [p. 122] over political choices or these decisions were determined together or individually). It is a credit to Duverger's understanding of the limits of cross-sectional analysis that he located each of his specific findings in the long-term social processes that shape it. Because these social processes have themselves changed, Duverger's caveats have proved more durable than his original findings.

Important as it was, *The Political Role of Women* had profound limitations as well. Its most serious difficulty was that although Duverger groped for a theory of the political or social relationships between women and men, he could not articulate one. Predating feminist political theorizing about sex and gender and avoiding Marxist interpretations of relations between the sexes, Duverger retreated uneasily to intrapsychic explanations, including men's warlike nature and the *mentalités* developed by women as a subordinate group. Similarly, he avoided discussions of regime and economic factors, perhaps because the research for *The Political Role of Women* was done under the aegis of the United Nations. For example, he describes Yugoslavia as having one-party elections but not as being an emerging state-socialist country riven with nationalist problems. Likewise, he mentions World War I far more often than World War II! From this volume the reader would never know that eight years before Duverger began his research, Europe was engulfed in a horrific war where more than 25 million Europeans died and a large proportion of the industrial capacity of the region was destroyed. The whole idea of European countries' reestablishing their political centers of gravity while simultaneously refashioning their economies is missing from Duverger's analysis. In this social context, women's electoral participation is more interesting and complex than Duverger's analysis conveys.

Feminist political theorizing on the distinctions between sex and gender responds to the theoretical emptiness that dogged the behavioralist approach. *Sex* focuses on the biological differences and similarities not just between women and men but also among groups of women or men; *gender* focuses on the social organization and meaning of these similarities and differences. This theorizing permits scholars to ask and attempt to answer the two questions Duverger did not have the vocabulary to articulate: How do women in their female bodies fit into the polity when gender itself is a complex constituent of the polity? And how can these patterns be compared across and within nations, institutions, social movements, and groups of people—indeed, across all the structures and processes of politics, broadly defined?

Each of these six newly published books on women, gender, and the political economy answers these questions differently. The four that emphasize politics (Alvarez's study of Brazilian women's movements in the transition to democracy, Watson's edited volume on Australian feminists' trying to make the state and its programs amenable to feminist policies and perspectives, West and Blumberg's wide-ranging anthology on women's participation in a variety of social movements, and Chou and the Clarks' volume on women in the Taiwanese legislatures) succeed most fully when they link individuals, the state, and society. The two books that emphasize economics (Tinker's compendium on the persistence of a special form of female poverty in the face of decades of development programs and Kardam's study of how three international development agencies have approached adding women and development to their agendas) succeed most fully when they link macroeconomic patterns of economic development, political processes, and the labor that women perform. In both cases, these links are crucial because they call into question theories of politics, participation, the economy, and labor that hold men's place to be both normative and universal. Without these links, authors and editors fail to provide the stereoscopic vision that attention to sex and gender can give to political analysis.

Sonia Alvarez' *Engendering Democracy in Brazil*, which analyzes the development and political consequences of women's movements during the Brazilian transition to democracy, provides a richly detailed and theoretically important volume on gendered patterns of interest intermediation during regime transformation. Her work demonstrates how the fraternal structure of democracy responds to women's organizing when civil society is not subservient to formal political society. Focusing on greater São Paulo, Alvarez recounts the political histories of two types of women's groups: *feminine* groups that seek to advance practical gender interests to solve immediately perceived needs and *feminist* groups that seek to advance strategic gender interests by advocating more fundamental changes in society.

One of the most contentious problems facing both types of women's groups during the movement from military rule to elite-administered democracy in Brazil was that of determining the proper relationship between these groups, political parties, and the state. In the beginning, when electoral politics was part of the opposition to the authoritarian regime, there was relatively little debate within women's groups over the propriety of electoral participation. But as the situation stabilized, and a center–Right coalition became powerful both nationally and in the state of São Paulo, Paulistas were deeply and bitterly divided over whether to keep their women's organizations autonomous or work with parties or the state. The parties both courted and wanted to coopt these groups. The parties of the Left, with their Marxist consideration of sex as a "secondary contradiction" and their masculinist organizational practices, also offered the programmatic reforms both feminine and feminist groups most sought. This dilemma became especially difficult as democracy regularized and political institutions swamped civil institutions as the vehicles of political participation and problem solving.

The solution for some individuals and groups was the *dupla militança* (double militancy) of party work and activism in autonomous women's groups. But even those who argued that women had to make a nonauthoritarian state in order to live in a nonauthoritarian regime had to come to terms with the often unpalatable compromises of party politics. The experiences of the women's councils and departments established with the return to democracy made the dilemmas of these compromises real. Party leaders underfunded these units and filled them with party women, who, though often feminists, were not representative of the range of autonomous women's groups. Nonetheless, with strong leadership such units occasionally succeeded in promoting women's strategic gender interests—as when Eva Blay, an alternate senator and former leader of the State Council on the Feminine Condition, led the fight against the government's attempt to implement an internationally funded family planning program in a way that would control, rather than expand, women's contraceptive choices.

Like the Brazilian activists, Australian feminists experienced a similar conflict over the propriety of participating in the state apparatus. Indeed, this conflict was central to women's movements in most democratic countries from the late 1970s through the 1980s. As Sophie Watson, editor of *Playing the State*, says in her introduction, part of this conflict originated in orthodox and New Left ideologies from which socialist feminists drew their initial theoretical sustenance. Watson nicely summarizes the transformation of views about the state among feminists in Australia and other English-speaking countries within the commonwealth. This theorizing moved away from the reified state of classical leftist analysis to a recognition that the relationship between women and the state was complex and historically varying.

Watson does not recount the history of the vitriolic debates among Australian feminists over whether feminists should participate in social change through the state, a participation that was often seen to undermine autonomous organizing and to lead to "mere reformism." At the theoretical level, Judith Allen engages this question in the essay, "Does Feminism Need a Theory of 'the State'?" But Allen is unconvincing analytically when she says that the state is not an important category of feminist analysis. At best, her argument should be read as a call for better theorizing about the state—theorizing not done in the service of adjectival feminisms.

The rest of Watson's volume provides examples of how Australian feminists have "played the state." Nowhere in industrialized democracies have feminists succeeded as well at the "artful game" of "turning the state back on itself" (p. 191). Recognizing that there was little tradition of nongovernmental, nonparty solutions to problems in Australia, a group of well-educated, middle-class, Labour party feminists entered state and national bureaucracies in the 1970s and set about creating, with the support of the Labour government, permanent governmental institutions to represent women's interests. At the national level the *femocrats* were remarkably successful. They used a dual strategy of creating a bureau committed to improving women's status within the office of the prime minister, while also establishing planning units (of varying seriousness and duration) within functional ministries.

The femocrats also helped to fund such feminist projects as women's refuges (battered women shelters). In "Interpretation of a Frontline State" Ludo McFerren, like Alvarez, documents the struggles of feminists to negotiate the forms of the relationship between women and the state. For example, because the national government initiated and funded a large-scale refuge program from 1975 to 1977, feminists in Canberra encouraged grassroots feminists to form a national organization to represent their interests. Women in the refuge movement resisted, however, and created a confederation instead. This confederation promoted the independence of the refuges but, without a national lobbying arm, also unwittingly created the situation where refuges became more dependent on state governments, rather than the national government.

While Australian feminists used the bureaucratic state quite effectively, gains in electing more women, regardless of ideology, and more feminists, regardless of sex, were less impressive. In this, the Australian case parallelled the experience of women around the world. Despite its general title, *Women in Taiwan Politics* is about the experiences of women candidates and legislators in a country whose yearly gross national product per capita grew from one hundred dollars in 1950 to seven thousand dollars in 1989. Like Duverger, these authors are successful at looking at women within Taiwanese local and national legislatures. They show that women, mostly from elite backgrounds, overcome cultural proscriptions against women's political participation by mobilizing resources from supportive natal and marital families. As legislators, these women are at least as good as men at constituent service, which includes promoting economic development projects, but are less active than men in introducing legislation.

This book is particularly interesting because Taiwan has a complex system of reserved seating for women in all legislatures. Two electoral systems are used to reserve seats for women. In the National Assembly reserved seats for women are elected by the membership of specified women's organizations that support the government. In all other legislatures the reserved seating process works in this manner: if women candidates do not win 10% of the seats in each multimember district, the female candidate(s) with the highest number of votes replaces the winning male candidate(s) with the lowest number of votes until women candidates comprise 10% of the members in that district. In practice, women usually win enough seats on their own to make the allocation of seats to women through the reserve process unnecessary. From comparisons with women's lack of success in winning executive offices, Chou and the Clarks argue convincingly that the reserved system has set both a floor and a ceiling on women's legislative success.

But like Duverger, the authors of *Women in Taiwan Politics* do not sufficiently integrate theoretical issues of gendered regime structures into their work. Taiwan is characterized by a military and bureaucratic authoritarianism that has only recently begun to change and whose effects on what must be considered at best quasi-democratic legislatures need further attention. One of the most important questions that arises from their work is the place of reserved seating systems in overcoming the gender inequalities structured into legislatures. Chou and the Clarks describe Taiwan's reserved seating system as "possibly unique" (p. 5) or "unique" (p. 81). In fact, Bangladesh, Egypt, and Pakistan have, or recently had, reserved seating for women; and India, New Zealand, and Zimbabwe have, or recently had, reserved seating on the basis of caste, ethnicity, or race. Taken together, these practices pose interesting questions about the nature of representative democracy, especially because most research on group representation in legislatures has focused on the cooptation of such groups in rubber-stamp parliaments. Representation based on one-person-one-vote is the norm in freely elected legislatures. Many bicameral legislatures also privilege regions (as a proxy for, e.g., language, ethnicity, religion), providing equal representation for regions of vastly differing populations as a partial resolution of historic regional conflicts. Seen in this light, reserved seating for women, castes, or races raises the same kinds of democratic questions as reserved seating for say, Wyoming, in the U.S. Senate. The question that needs to be asked in future research is, Why do legislative arrangements privilege some social cleavages over others, and with what results?

Of course, reserved seating is just one of the mechanisms used to promote greater physical and ideological representation in legislatures. The much more common approach is ticket balancing. Carol Schmid's chapter, "Women in the West German Green Party" in West and Blumberg's collection *Women and Social Protest* provides a detailed discussion of how the Greens, who believed both in virtual representation and compensatory democracy, developed the policy of equal female and male representation on party lists. This practice transformed the nature of gender intermediation in political parties all over Europe by both responding to, and increasing competition for, women's votes.

An important aspect of the West and Blumberg volume is that the chapters investigate women's participation in a large number of social movements, using a wide range of methodologies. The editors divide the book into four parts dealing with women's participation in different types of social movements: grassroots movements for economic survival; racial, ethnic, and nationalist struggles; social-nurturing and humanistic protests; and women's rights activism. This typology, based on the different configurations of the nexus between women's identities and power politics, begins an important endeavor to conceptualize social movements by goal, location, and strategy, as well as opportunity structures and resources. The contributions also use an impressive variety of methods. As sociologists, the editors have more easily avoided the overemphasis on the nation-state that characterizes comparative politics as a field in political science. But their success derives from more than just emphasizing a wide variety of units of analysis. The editors have assembled a series of essays that use individual- and multiple-case studies, collective biography, historical analysis, quasi-experimental designs with extensive statistical analyses, and spatial analysis to examine women's and men's forms of social protest.

None of the articles employs economic theories or statistical techniques typical of economic analysis, however. Conventional economic theories and techniques are out of favor in social movement studies for two reasons. First, the theories that linked economic conditions directly to unrest were not supported by evidence; the relationship between economic conditions and protest is mediated by political, cultural, institutional, and individual considerations in complex ways. Second, the concepts of "unrest" and "protest" have themselves been questioned as too universal, too linked to theories suggesting that mass action is unusual, and (for women) too connected to men's access to public space and economic resources.

Irene Tinker's excellent anthology *Persistent Inequalities* looks not so much at protest as to the persistence of specifically gendered types of poverty in the less-industrialized world. As such, the volume contributes to the information necessary to build the more accurate models of political action that have replaced purely economistic ones. The collection is a festschrift in honor of the twentieth anniversary of the publication of Ester Boserup's *Women's Role in Economic Development* (1970). Like Duverger's work in political participation, Boserup provided the classic text that introduced the study of women in economic development to the development field. Her research argued that in systems of shifting cultivation, which she defined as *female farming*, women's economic input was at least as important as men's. With the advent of plow cultivation, which she defined as *male farming*, women's economic contribution was marginalized by male monopolies over the technologies of production.

One of the great values of Boserup's work was that it provided an analysis of the importance of women's contribution to economic development in the theoretical language of economic efficiency used by neoclassical economists. In "Gender and Justice in Economic Development," Jane Jaquette perceptively analyzes the justice claims in various development theories. She writes that "Boserup's argument speaks directly to this market conception of a merit claim for women. [Boserup asserts that] in the historical past, women were not only equal in status [with men], they were equally productive. . . . It is differential access to technology that has deprived women of their claim to equal productivity" (p. 61). From this point of view, the failure to include women in development schemes robbed less-developed nations of potential increases in economic growth. Boserup's argument about the economic inefficiency of overlooking women's contributions to economic development—more than arguments made from claims based on women's equality with men or their special needs—motivated the inclusion of women in development programs funded by international agencies and national planning commissions.

Boserup's work was also criticized on similar grounds as was Duverger's. Her theories were silent about forms of male domination that could not be explained by the economic impact of differential access to agricultural technology. She failed to develop a theory of how patriarchal social and political relations changed and modernized in ways not derivative of economic relations. Similarly, as Vina Mazumdar and Kumud Sharma discuss in "Sexual Division of Labor and the Subordination of Women," Boserup did not situate her analysis of male and female agricultural systems in a world economic system constituted by an elaborate sex/gender division of labor in every sphere of production.

Nüket Kardam's *Bringing Women In* examines how three development agencies (the UN Development Program, the World Bank, and the Ford Foundation) responded to the opportunities, and met the challenges, of adding women to economic development programs. Although excessively reportlike in its presentation, this book provides an interesting examination of the role of institutional culture in issue adoption. Kardam found that the Ford Foundation, with its mission to promote innovative, knowledge-based solutions to problems and its decentralized decision-making system, was most responsive to the demands for inclusion of women-sensitive programs within its development programming. The Ford Foundation also had the least money to spend and the fewest outside actors to satisfy. The World Bank, with enormous resources but the need to demonstrate economic efficiency, made a moderate level of commitment to women and development projects. The UN Development Program, having to coordinate funders, projects, and country officials, had the lowest level of commitment.

Kardam's research would have benefited from drawing wider intellectual boundaries. Her focus on the organizations' relations in the international development regime made her miss the important feminist epistemological questions her study raises. For example, Kardam gives a measured, but ultimately devastating, description of the intellectual culture of the World Bank, where women's equity issues are unwelcome because "they are not easily quantifiable and introduce many unknowns" (p. 72). But she does not draw the parallels between the World Bank's reliance on one type of analysis and stereotyped views of Third World women as wedded to old-fashioned methods of problem solving. Who is modern, and who is traditional, in this scenario? Without explicit theorizing about the gendered construction of knowledge, Kardam cannot answer these questions.

Read as a conversation, these books suggest three directions for future research. First is the need to pay more attention to gender as a social and political cleavage. In particular, they suggest that it is crucial to examine how gender issues are mapped onto systems of interest intermediation. Second is the need to do more comparative research on the political activism of groups of both differently situated women within countries and similarly situated women across countries. Together, the similarities and the differences among groups of women illuminate the force of social structures and the power of individual agency in defining women's political engagement. Third is the need to give greater attention to the theoretical and empirical issues of sex and gender in the system-level studies of comparative politics, especially in the analysis of regime characteristics and regime change. In other words, the study of sex and gender in comparative political analysis, like the study of other topics within the field, needs to include a focus on individuals, institutions, and regimes. These six books contribute significantly to the foundations of this research.

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POLITICAL THEORY

Pragmatic Liberalism. By Charles W. Anderson. Chicago: University of Chicago Press, 1990. 220p. $27.50.
Political Criticism. By Ian Shapiro. Berkeley: University of California Press, 1990. 338p. $35.00.

These two books are in important respects similar in intent, even though each employs different argumentative strategies and arrives at quite different conclusions. They are alike in that both are committed to criticizing the abstractly formal character of much of modern political theorizing (as found in, e.g., Rawls's fiction of an original position or Habermas' ideal speech situation). Each, moreover, proposes a less hypothetical and ostensibly more muscular alternative—Anderson opting for *pragmatic liberalism*, Shapiro for *critical naturalism*.

On Shapiro's telling, a good deal of contemporary theorizing amounts to a "turn away from Neo-Kantian political theory" as practiced preeminently by John Rawls. That form of theorizing gives pride of place to abstract, ahistorical, and dissociated individuals who come somehow to subscribe to universal principles under which each can pursue a personal vision of the good life. Shapiro's *Political Criticism* consists, in the main, of explications of the views of five critics of "Kantian" theorizing—and a motley crew they are. If politics makes strange bedfellows, anti-Kantian political theorizing makes even stranger ones. On his procrustean bed Shapiro places Richard Rorty, Michael Walzer, Allan Bloom, Alasdair MacIntyre, and J. G. A. Pocock (chaps. 2–6, respectively). What connects and makes kinsmen of this disparate group, Shapiro asserts without argument, is their shared antipathy toward neo-Kantian formalism. This seems to me to be the thinnest of threads with which to connect these thinkers, nor do I think that Shapiro succeeds in making his case in those terms. Three (Walzer, Rorty, and MacIntyre) fit the bed fairly well; but Pocock does not, and Bloom belongs on an altogether different bed. *Contra* Shapiro, Pocock, for example, recovered or reconstructed an "Atlantic republican tradition" not in order to provide a workable communitarian alternative to liberalism but to counter certain historiographical claims about the hegemony of liberal ideas in England and colonial America. Pocock's targets are, accordingly, historians like the late Louis Hartz and C. B. Macpherson, and not philosophers like Rawls (still less, Kant).

Such objections aside, however, Shapiro has written an interesting and important book. Its interest and importance lies in its very clear expositions and cogent criticisms of the views of these five theorists and the brief sketch (which he promises to fill in at a later date) of an Aristotelian critical naturalist alternative to neo-Kantian and antifoundationalist approaches alike.

If Shapiro's hero is Aristotle, Charles Anderson's is John Dewey, along with other exemplars in the pragmatic liberal tradition. Anderson is highly critical of the modern turn toward such abstract, ahistorical, and hypothetical constructs as the competitive free market (à la Milton Friedman and Robert Nozick), the social contract (in its Lockean and Rawlsian versions), and other simplisms. If liberalism is to be saved from itself (e.g., from the excesses of simplification and abstraction favored by too many of its modern defenders) it must take a practical, or pragmatic, turn that recognizes the changing historical contexts and varying kinds of social and institutional complexity that characterize modern societies. In the discipline of practical political reason, principles are not abstract and universal but, instead, derived from a workable vision of the fair and efficient functioning of disparate social practices and institutions. This vision is not an a priori philosophical construct but is instead arrived at pragmatically, through debate and dialogue about the place and purpose of particular institutions in their larger social setting. Anderson's appropriation of Dewey, Veblen, and others is richly suggestive, and points to political possibilities and nuances too often overlooked in arid confrontations between defenders of liberalism and advocates of communitarianism. Far from being dichotomous entities, these are, in Anderson's view, compatible and complementary strands in the pragmatic liberal tradition: *communitarian liberalism* need not be an oxymoron.

Even so, Anderson's analyses and criticisms are arguably directed less at academic political theorists than at planners and policymakers (the latter too often taking their cues from the former, with actually or potentially disastrous results, e.g., deregulation, privatization, and other theory-driven policies). Anderson warns not against the dangers of theorizing per se but against the Sirens of abstraction and oversimplification. Plain in its prose and powerful in its arguments, Anderson's *Pragmatic Liberalism* is a book to reckon with.

University of Minnesota TERENCE BALL

The Communicative Ethics Controversy. Edited by Fred Dallmayr and Seyla Benhabib. Cambridge: Massachusetts Institute of Technology Press, 1991. 378p. $37.50 cloth, $16.95 paper.
Critical Theory in Political Practice. By Stephen T. Leonard. Princeton: Princeton University Press, 1990. 295p. $35.00.

Although these books relate most specifically to the literature of what is termed critical theory, they raise issues exemplifying the ambiguous situation of political theory more generally conceived and, indeed, the discipline of political science. Political science and political theory have, historically, had their roots in critical engagée enterprises but now find themselves institutionalized and professionalized with little clear sense of their relationship to political life.

One of the great ironies in the history of social thought is how Marx's critical theory was transformed into an apology for dogmatic orthodoxy. Stephan Leonard suggests that no less an irony (although played out on a smaller stage) is how the modernist critical theory of the Frankfurt school (aimed at salvaging the emancipatory intentions of Marx and applying them to the conditions of the twentieth century, as well as postmodernist, critical theory (associated, especially, with Foucault), has become little more than an idea and a "form of academic, intellectual introspection."

A theory defined by the goal of bridging the gap between theory and practice and arguably representing "the most perspicacious extant understanding of what social inquiry is and must be" has, in its various forms, become a quintessential example of "shunting practical questions to a secondary status." It has failed to realize its own aims, make good on its commitments, and even to make clear how it could be related to concrete political practices. Leonard offers both an explanation and a solution.

The explanation is that critical theory became absorbed with a "metacritique of modernity" and "metatheoretical preoccupations" that subverted its practical intentions. Although it emphasized, for critical purposes, the "historicity and contextuality of our knowledge," nevertheless, from Marx through Habermas, it either paradoxically sought some version of universal reason (or normative universalism) and truth to ground its claims and justify its critical posture or else fell into a postmodernist skepticism like Foucault's, abandoning the very idea of reason and thereby undercutting its own critical project. It is, in the end, the metatheoretical turn and the tyranny of universalism that is politically stunting for critical theorists and explains the "gap between the promise and the realization of their own ideals."

It might well be argued that this fate of critical theory is represented in the extensive controversy that has surrounded Habermas' *Theory of Communicative Action* and the attempt to find in the structure and logic of speech a kind of immanent transcendentalism. This controversy has now, on both sides of the Atlantic, become an important part of a more general meta-ethical debate about the possibility of a postposivitivist cognitive ethics. Since much of this discussion has taken place in Germany, Fred Dallmayr and Seyla Benhabib have compiled nine essays by German-speaking authors, including Habermas and Karl-Otto Apel (the "foremost spokesmen of communicative ethics"), and representative defenses and critical considerations of their positions.

Dallmayr's concise but comprehensive introduction does a splendid job of clarifing the issues in the controversy, indicating its place in the larger contemporary debate between ethical rationalism and skepticism, distinguishing the more Kantian foundationalist approach of Apel from Habermas' less transcendental formulation, and explicating the positions and arguments of the respondents. Behabib's afterword explores more fully the "provocative interaction" between the Anglo–American and Continental traditions in ethical theory. The focus is especially on the criticisms of universalism in discourse ethics that might be—or have been—urged by various neo-Aristotelian and neo-Hegelian theorists, including the hermeneutical approach of Gadamer, but with a view toward reconciling some of the characteristic oppositions, such as that between universalism and historicity.

Following through the complexities of this metatheoretical debate indicates just how far the aspirations for universalism and the search for the authority of knowledge have been alienated from the original practical concerns of critical theory and political theory in general. The solution, Leonard argues, is to redeem critical theory through (1) an "immanent critique" that puts into relief its aspirations and latent possibilities and (2) a "reconstruction" of certain social theories representing critical theory in practice and making good its aims (to which the largest part of the book is devoted).

Leonard provides a valuable, extended examination of dependency theory, Paulo Freire's "pedagogy of the oppressed," liberation theology, and certain aspects of feminist theory. What these theories have in common, he claims, is that they are "self-consciously tied to particular historical circumstances and practical contexts" and are "part of struggles where people are emancipating themselves," rather than a merely "esoteric and purely academic ideal."

What must be retained from modernist critical theory is its emancipatory goal and its voice rejecting transcendental social truth. While postmodernism provides the required historical or contextual stress, its skeptical antifoundationalism is as dangerous as—and in some sense merely the obverse of—alienated univeralism and makes it difficult to "take a stand that is in solidarity with those who are oppressed." Leonard argues that the critical theories in practice embody these requirements and thereby both vindicate critical theory and provide a model for assessing critical–theoretical endeavors. It is not enough, however, that they are simply critical and practical. If they are to be more than ideologies, they must contain a "metatheoretical self-understanding that preserves the demand for solidarity and the recognition of plurality and difference."

This is a serious book that deserves to be taken seriously. Leonard has taken an important step and confronted the fact that there is no philosophical solution to the problem of theory and practice. The recent literature of academic political theory is replete with work that like Leonard, seeks to defend an idea of critical theory that avoids foundationalism and yet circumvents relativism, thereby reconciling historicity and truth. But in nearly every case, the argument rests on a metatheoretical twist and on an epistemological strategy that avoids the concrete issue of the existential relationship between theory and practice. Although Leonard is offering still another metatheoretical mediation, he is also saying, quite simply, that in themselves, such academic constructions are sterile. Critical theory without a practical dimension is not critical theory. There are, however, he suggests, certain situated engaged theoretical practices that either go far toward realizing the idea of critical theory or point toward such a realization.

The examination of critical theory in political practice is an interesting and distinctive contribution, but Leonard is too modest and too painfully self-conscious about the inherent limitations of his general endeavor. He deeply feels the paradox that his project is an "academic discourse" when he is claiming critical theory must be more and that it is a metatheoretical project—"a theory about theories"—when he is arguing that metatheoretical diversions are a large dimension of the problem.

The problem with metatheory is not that it is inherently inauthentic but that it pretends to be something it is not—either theory or practice. Leonard does not fall victim to this delusion or temptation, but he does ultimately shy away from the issue that at some level, he realizes, lurks behind his discussion. This is the issue of the relationship between academic and political discourse in both its generic and specific dimensions.

Whereas Critical Theory is an institutionalized academic discourse and one that is largely metatheoretical, critical theory in political practice is a stipulative functional category for classifying certain practical or quasipractical discourses. Leonard is clear enough about this difference in principle; but in the course of his discussion, the distinction begins to blur, since the latter validates the former and the former is manifest in the latter.

Leonard begins his book with a confessional testimony to his sensitivity to being a privileged Western professional white male while having political commitments that involve "a desire to reach out to those who are engaged in . . . day to day empowerment struggles" and "who now suffer damaged lives." In critical theory he saw the possibility of reconciling the two; yet he realizes that his book is primarily written for, and will be read by, a largely academic audience.

His mediation is, in the end, a conceptual one whereby he underwrites practical critical theory by eliciting from it an "understanding of knowledge and human identity" that accords with the best instincts of academic critical theory. He wants at once to make theory practical and keep it pure. At best, he hopes to provide "analytical insights that might allow some—even if they are 'only' theorists—to begin to bridge the gap between critical theory and concrete, historically contingent political struggles."

There is a good deal of pathos in these reflections. They do not, I think, carry us very far in understanding or dealing with the concrete structural problems inherent in the relationship between the American (and maybe Western) university and politics and how personal political commitment is sublimated in philosophy and professional activity. But they—and as the book as a whole—bring us, both in spirit and substance, to a point where we cannot retreat from the dilemma without conscious repression and bad faith.

State University of New York, Albany JOHN G. GUNNELL

Nietzsche and the Politics of Aristocratic Radicalism. By Bruce Detweiler. Chicago: University of Chicago Press, 1990. 242p. $24.95.

Nietzsche constantly complained about being misunderstood, but he uncharacteristically once embraced George's Brandes's suggestion that his position was an "aristocratic radicalism." Bruce Detweiler agrees that Nietzsche's attempt to formulate a unique modern position in political thought, alongside "aristocratic conservatism" and "egalitarian radicalism," is best captured by that phrase; and he has written a book-length defense of such an interpretation. For the most part, he restricts himself to interpretive issues, quarreling mostly with those like Walter Kaufmann and Peter Bergmann, who argue that Nietzsche was an antipolitical or apolitical thinker, and those like Mark Warren, who argue that Nietzsche's attack on the nihilistic consequences of the Western tradition is most consistent with a politically pluralistic or liberal democratic position. (There is some brief reference to the European literature on these topics but no discussion of the relevant work on similar themes by Michel Haar, Philippe Lacoue-Labarthe, Henning Ottman, or the like.)

Detweiler argues first that Nietzsche's radical *perspectivism* and his attack on the traditional notion of truth are not inconsistent with the possibility of a general political position. A perspectival approach to classifyings and evaluatings entails neither any framework relativism (wherein we could not *understand* other perspectives) nor any surefooted claim that such classifyings and evaluatings must be false. Nietzsche's own position is said to be adopted "in the most tentative of terms," thus allowing "tentative convictions about relative adequacy" when assessing perspectives (p. 27). This adequacy appears to be a function of whether a perspective is "more honest and searching, more comprehensive and more noble" (ibid.). The discussion here is brief. The author only suggests that some evaluative position might be logically compatible with Nietzsche's perspectivism. He does not develop or defend Nietzsche's own position in those terms.

According to Detweiler's Nietzsche, there are only a very few capable of forming, creating, animating, legislating, or commanding such perspectives. Such rare artist–legislators form the center of the political story told here. Politics, as Detweiler's Nietzsche understands it, has a great deal to do with creating the conditions under which such noble types are possible and can thrive and with hastening the ever-accelerating collapse of the antiaristocratic, modern, egalitarian political culture. The strength of the book lies in Detweiler's marshaling of evidence to support the claim that Nietzsche's picture of a postnihilistic aristocracy was a decidedly political one, and in his detailed criticisms of contrary, antipolitical readings.

Thus, on this account, society *as a whole* "should be dedicated to the promotion of the highest type rather than to the universal good or the common goods" (p. 101). This highest type is a modern extension of the "master," or pre-Christian, morality identified in *On the Genealogy of Morals.* Its virtues include a robust egoism; indifference to the effects of one's deeds on, or their general acceptability to, others; a courageous self-reliance in the face of "the death of God" or the impossibility of any sort of cosmic reassurance about the worth of one's deeds; and some ability to integrate, create a structure for, or in some way harness and harmonize the various "drives" contending for power in one's internal life. Such a virtue is characterized as "Dionysean," although the author carefully differentiates Nietzsche's early, somewhat "metaphysical" use of that term in *The Birth of Tragedy* from the "self-forming" ideal that emerged in later works.

The book leaves a large question unanswered—perhaps the central issue in any discussion of Nietzsche's politics. It may be a matter of fact that the historical Nietzsche did claim that the self-forming, heroically courageous, independent individual described in Detweiler's later chapters was the proper telos of all political life and that the historical Nietzsche did vigorously propose political goals, concerned himself with antidemocratic politics, and encouraged a grand politics—all as if such enterprises were central to the very possibility of such a type. But are they? Why would the new "master of the earth," so contemptuous of the herd, want, or even need, to *rule* the herd? Is it some sort of anthropological truth that the everywhere-motivating will to power requires actual political domination? Is it a matter of simple prudence? Since the herd will not leave him alone, he must preemptively create the political world that will allow his survival? What sort of world is that? Why would not the traditional refuge of esotericism suffice?

And none of these possibilities is consistent with the "politics" of Zarathustra, who overflows with *love* of man and is unable to remain in his cave, with animals. He needs men, even if already almost "last men." Even if the politics are "aristocratic" and the master needs only a certain few, the nature of that need and its implications for Nietzsche's supposed "individualism" remain important and still unanswered questions.

University of California, San Diego ROBERT B. PIPPIN

Compromise and Political Action: Political Morality in Liberal and Democratic Life. By J. Patrick Dobel. Savage: Rowman & Littlefield Publishers, 1990. 215p. $38.00 cloth, $18.95 paper.

The Moral Imagination and Public Life: Raising the Ethical Question. By Thomas E. McCollough. Chatham: Chatham House Publishers, 1991. 191p. $14.95 paper.

These two books would enable the traditions of communitarian and liberal political thought to respond to some standard criticisms leveled against them, particularly the difficulties both have in addressing the most serious practical problems of (American) political life. McCollough searches for a communitarian public ethic that attends to contemporary public policy as well as currents in American history; one that would revitalize the public realm as a place of ethical involvement for ordinary citizens. He endeavors to explain and remedy the current alienation from democratic politics felt by many Americans, paradoxically, when post-Communist nations seeking to renew themselves aspire to emulate many of its features. Dobel writes from the perspective of "responsible political actors"—he himself is chairman of a County Ethics Commission—and would teach them "about how individuals should hold moral beliefs in political life." (p. 3) His treatment of the ethics of compromise seeks to show how and why liberal principles may inform a successful politics. These volumes commendably eschew professional jargon wherever possible and avoid the solipsism of many academic debates. Their authors have written for intellectually curious generalists devoted to bettering our political life.

The non-technical approaches employed display both virtues and drawbacks. The virtues stem from the range of issues discussed and each author's interconnection of them. McCollough weaves together a critique of Lindblom's models of rational public policy formation; sympathetic accounts of a 1968 Duke civil rights vigil and Earth Day-1970; narratives of the American founding; complaints about utilitarianism, liberalism, and pluralism for their contribution to corrosive individualism, amoral scientism, and narrow rationalism; nostalgia for medieval community; reliance on the conceptual categories of Tonnies and Kierkegaard; appeals for deliberative democracy, and a call for Christian-based vision of moral community. Dobel's historical and normative scope is less wide-ranging, but he deftly integrates accounts of the way American politics has dealt with the legacy of slavery; critiques of Aristotle, Machiavelli, Weber, and communitarians; adaptations of Rawls; and analyses of the logic of prudence and justification. The drawbacks stem from both authors' inability to give any one of these topics their due and the inevitable distortions that come from severe simplification. For example, Dobel criticizes Aristotle (via MacIntyre and Beiner) for not being self-critical and McCollough interprets the Constitution as a betrayal of the ideals of the Declaration of Independence—each using only a few sentences to justify these sweeping claims.

But what about their arguments? While each extends and elaborates communitarian and liberal positions, neither has removed their limitations. McCollough makes a good case for the view that major strands in America's political culture and dominant modes of argumentation in ethics and public policy tend to marginalize the idea of community as a commanding ideal which, in turn, favors the political reason and power of elites over ordinary citizens and isolates ethical concerns from our public discourse. But he never seriously confronts the political phenomena which have given our dominant political ethos its persuasive force—namely, the character of conflict in American society. The clearest examples McCollough cites of "the communal point of view" having a beneficial, practical, and coherent effect are its instantiations by branches of government whose linkage to any participatory community is by definition suspect. One is the institutionalization of fluoridation (p. 59) which was primarily promoted by *regulatory* agencies of local and state governments. McCollough's other major example of a successful, communally based public policy is the reportedly successful institution of a *judicially* initiated school busing plan for Charlotte, North Carolina (p. 112f.). Moreover, busing may have worked in Charlotte, but nation-wide it has generated more opposition than approval. Finally, although McCollough's endorsement of the idea of a non-denominational "public church" as a vehicle for rekindling the desire of all citizens to believe in an American moral community is well-taken, his failure to consider the role of Catholic and various fundamentalist churches in the abortion controversy detracts from the authority of his position. McCollough has much faith in a basic linkage between participatory citizenship and the creation of a moral community, but what are we to think about Americans' prevailing ways of demonstrating their ideas of moral community? If we find them uncongenial, how do we determine which moral community is the really good one? In order to embrace a new ethos of community that will inspire and harmonize the American citizenry, we need to know more about how this new ethic can accomodate the conflictual differences that both enrich and divide the two hundred fifty million Americans who hail from all corners of the globe.

Dobel's discussion of political compromise would provide such an ethic. He believes that "compromise" has been given an unnecessarily bad name. It is inherently associated with neither immoral conduct nor *realpolitik*. To the contrary, it is a necessary tool of liberal and democratic politics that can become an instrument for both preserving the moral integrity of political actors and building an ethical political community. Rather than urging us to maintain political community by removing issues rooted in fundamental moral and religious commitments from the agenda of liberal democracies, as does Rawls's current work, Dobel believes that liberalism, via the art of political compromise, can help us accommodate them. So that "compromise" can fulfill the function Dobel ascribes to it, he infuses ethical substance into its ordinarily technical meaning. For Dobel, not any compromise will do. He is wary of how compromise can be used by conservatives (e.g. Burke and Oakeshott) to preserve the status quo and by amoral politicians to undermine the political efficacy of critical, moral principles. But he stresses that unwillingness to compromise one's principles can encourage coercive, if not violent, behavior in public life and that presupposing a substantive notion of community as a basis for political justification inhibits the moral and political reflection of individuals. He therefore offers the normative idea of a "good compromise" as the key for practical political ethics. It is a "principled compromise" which produces an "integrated solution," awarding each of its parties equal Rawlsian respect while producing a mutually advantageous outcome. Dobel's historical examples of such compromises include the Missouri Compromise, the Civil War Amendments, and Lyndon Johnson's civil rights strategy (beginning in the Senate and continuing through his Presidency). But can one really get from compromise to community? Lasting, worthy compromises typically depend on practical conditions and ethical values which haven't been subject to compromise. If acquiring the art of compromise would improve the conduct of American politics, the acceptance of the liberal principles that can insure its successful practice must already be an accomplished fact. In other words, even as Dobel would seek to demonstrate a new political ethic of compromise, its persuasiveness depends on a satisfactory answer to prior questions about the coherence of liberalism as a political theory and its correspondence to political reality.

Although these authors approach the problems of ethics and politics from fundamentally opposed theoretical standpoints, they share common political goals. Both would have America become more egalitarian, participatory, enlightened, and democratic. Unfortunately, in these days of scarcity, what communitarianism there is the United States seems to subvert rather than promote a democracy of free and equal citizens, while liberalism continues to weaken as an effective political ideal. If we are to understand better how to make compromise a source of community, and ideas of moral community new ideals for public policy, we may need to abandon the meta-ethical positions of liberalism and communitarianism. Freed from these theoretical moorings, we may be in a better position to diagnose the ethical and practical elements of our new world order, which makes these authors' laudable convictions sound like sirens from the past.

Hunter College, CUNY JOHN R. WALLACH

The Nature of the Right: American and European Politics and Political Thought since 1789. Edited by Roger Eatwell and Noel O'Sullivan. Boston: Twayne, 1990. 203p. $29.95 cloth, $14.95 paper.

Reactions to the Right. Edited by Barry Hindess. New York: Routledge, 1990. 193p. $54.50 cloth, $19.95 paper.

The Political Economy of the New Right. By Grahame Thompson. Boston: Twayne, 1990. 216p. $29.95 cloth, $14.95 paper.

The Political Theory of Conservative Economists. By Conrad P. Waligorski. Lawrence: University Press of Kansas, 1990. 260p. $29.95.

Nearly 40 years ago, at the peak of McCarthyism, Richard Hofstadter referred to the American extreme Right as a species of "pseudoconservative" dissent "because its exponents, although they believe themselves to be conservatives and usually employ the rhetoric of conservatism, show signs of a serious and restless dissatisfaction with American life, traditions, and institutions" (*The Paranoid Style in American Politics and Other Essays*, [1979], p. 43). Associated with this curious mixture of conservatism and radicalism and with more violent radicalisms of both the Right and the Left, he suggested, was the "paranoid style" of mind whose main elements included a hyperrational awareness of "a 'vast' or 'gigantic' conspiracy as *the motive force* in historical events" and a crusading "will to fight things out to the finish" rather than compromise with the evil conspirators (pp. 29, 31; emphasis original). Pseudoconservative paranoia erupted during the early 1950s among Taft Republicans, who contended that godless communists had infiltrated the federal government and other key institutions, including universities and the mass media, as part of a sustained conspiracy—most evident in Roosevelt's New Deal—to destroy market capitalism, undermine traditional religious and moral values, and, ultimately, transform America into a cog within the Soviet empire. This fringe movement gained ground through the next two decades, achieving a temporary national prominence during the 1964 Goldwater presidential campaign. Increasing numbers of affluent whites and lower-middle-class fundamentalists apparently became persuaded that the eastern Republican establishment had capitulated to, and/or was too incompetent to defend the American way of life from, communists, socialists, and their possibly unwitting allies, variously labeled welfare democrats, Keynesians, and liberals.

By 1980, with the election of Reagan, the pseudoconservative moment had arrived. During the 1980s, the New Right began the attempt to "repossess" America and the federal government from liberals and their ilk. Their agenda, more or less continuous from Taft to Reagan, may be reduced to three main elements: no compromise with the Soviet empire as long as it subscribes to godless communist or socialist ideals; restoration of traditional religious and moral values by means of suitable amendments and/or judicial decisions (e.g., permitting religious instruction in the public schools) and state and local government sanctions (e.g., against pornography and abortion); and liberation of market capitalism from arbitrary government regulation, again with the help of suitable amendments and/or judicial decisions (e.g., limiting federal taxation, requiring a balanced budget, and limiting welfare expenditures). Moreover, in attempting to carry out this agenda, their uncompromising paranoid style has led to heightened ideological conflict in American politics, despite the fortuitous détente with the Soviet Union. As Patrick Buchanan puts it: "Our political and social quarrels now partake of the savagery of religious wars because, at bottom, *they are religious wars*. . . . Someone's values are going to prevail. . . . The only questions are whose religion, and whose morality, shall we consult?" (*Right from the Beginning* [1988], pp. 337, 342; emphasis original). Perhaps the "kinder and gentler" Bush administration is moderating this divisiveness to some extent, but the question must remain open for now.

Hofstadter's term *pseudoconservatism* underscores the point that the New Right morality is too radical to be truly conservative in the classical sense of Burke. This raises the difficult general issue of the nature of the Right, including what distinguishes it from the Left.

Roger Eatwell, in five chapters of *The Nature of the Right*, suggests that the Right, at least in the period since the French Revolution, includes five styles of thought, not necessarily compatible, that have emerged historically in response to liberalism and socialism–Marxism. More specifically, reactionary and moderate right-wing styles emerged in response to certain eighteenth-century liberal notions of individual liberty and perfectionism; radical and extreme styles emerged in response to nineteenth- and early twentieth-century socialist ideals of social and economic equality; and the New Right emerged during the 1970s and 1980s in response to the perceived incompetence of socialist and left–liberal administrations (pp. 63, 74). Five remaining chapters, by as many authors, illustrate the five styles by discussing, Charles Maurras's royalist critique of French republicanism, moderate British and American conservatism, the "conservative revolutionaries" of pre–World War II Germany, fascist conspiracy theory in post–World War II Europe, and New Right views of civil society in Europe and America, respectively.

Despite his brief remarks about the importance of ideology and "the marketing of political ideas" (p. 26), however, Eatwell never clarifies why these five styles of thought are properly classified together under the right-wing label or why it is useful to think in terms of right and left. His approach is reminiscent of Samuel Huntington's well-known "situational definition" of conservatism, according to which true conservatism is a "positional ideology" involving "the passionate affirmation of the value of existing institutions," whatever their content (*APSR* 51:455, 468). But at least two problems bedevil Eatwell's analogous situational definition of the Right. First, the nature of the Right remains ambiguous until we clarify the content of the left-wing challenge to which it is reacting in the given context. Second (and perhaps more important), Eatwell's extension tends to hide the fact that these five styles of thought are really quite different mixtures of conservative and radical elements. Huntington argued persuasively that radicalism is the opposite of conservatism, the essence of the former being "criticism of existing institutions," whatever their content (p. 458). In his view, neither conservatism nor radicalism per se is committed to normative ideals of any particular content. These isms are merely opposing attitudes toward existing institutions so that depending on the situation (in particular, the nature of existing institutions), each is compatible with many substantive ideals, including those associated with this or that version of liberalism (pp. 460, 472–3). It follows that support or criticism of any normative political theory will, except in limiting cases, involve a mixture of conservative and radical attitudes toward actual institutions. Of course, alternative theories might usefully be classified as moderate or extreme according to whether they imply piecemeal or revolutionary institutional change relative to the given status quo. If existing institutions are basically liberal, for example, then such a classification would call attention to the similarities between different types of moderate views (e.g., progressive conservatism and gradualist liberalism), as well as to the differences between them and radical views that are nonliberal by definition in this context. But any such classification by itself tells us little about the justification of competing political theories and their respective ideals. Moreover, its contextual validity must disappear as existing institutions change. Perhaps for these reasons, it can hardly be expected to correspond to any popular left–right spectrum.

Grahame Thompson, in *The Political Economy of the New Right*, argues that the Reagan and Thatcher governments were in practice constrained to adopt moderate economic policies at odds with their radical official postures. He suggests that New Right political economy involves four distinct doctrines, namely, monetarism (Friedman); supply-side economics; economic neoliberalism, including neo-Austrianism (Hayek); and the public choice approach (Buchanan), with two common features: "celebration of the virtues of the market, and . . . antithesis to the idea of a Keynesian welfare state" (p. 2). Building on earlier work, he concludes that the New Right administrations did not make radical changes on the basis of these doctrines, despite much official rhetoric to the contrary. For the most part, these governments merely introduced some less direct forms of market regulation, tinkered with the broadly Keynesian fiscal and monetary policies of earlier governments, and fostered substantially more inequality. The high economic growth observed in both the United States and Britain during the 1980s resulted not so much from monetarist policies or supply-side incentives to producers, for example, but more from continued government spending and an explosion in consumer demand, facilitated by easy credit and income tax cuts for the rich, during a normal cyclical recovery from the recession of 1979–81. More generally, Thompson seems to be saying that it is unhelpful to deploy labels such as New Right and Keynesian on the presumption that a radical ideological switch might empower us to alter the fundamental direction of the economy. Rather, we should recognize that workable policy ideas are largely ad hoc, piecemeal responses to complex and ill-understood changes in the economy itself.

Like Thompson, Conrad Waligorski is interested in the political economy of the contemporary Right. In *The Political Theory of Conservative Economics* He offers numerous insights into the normative political theory, which he claims is shared in broad outline by many influential economists today, in particular, Friedman, Hayek, and Buchanan. Despite its radical libertarian elements, he describes the theory as conservative for its insistence that human reason is too weak to understand or reform the web of conventions whose spontaneous evolution makes social order possible, its sanctification of certain contingent forms of inequality as inseparable from the spontaneous social order, and its related mistrust of democratic politics and political equality. In his view, the theory is inadequate because it arbitrarily selects certain private property conventions and merely takes for granted the optimality of a spontaneous market order based on them. It seeks, above all, to preserve this market order to the neglect of any individual's fate; and it systematically distorts and impoverishes traditional political ideas of freedom, equality, democracy, justice, and community by forcing them to conform to the market vision. Thus, freedom, for example, is reduced for the most part to following the spontaneous rules of the market. Despite the fact of unequal bargaining power among its participants, the market is noncoercive, "because it is impersonal and spontaneous and its results are unintended, unforeseen, and undirected by any one person" (p. 61). Although defenders of this sort of theory are not without replies to many of his objections, Waligorski nicely brings out some of the tensions between its conservative and radical libertarian elements. For example, from the Huntington-type perspective, true conservatism defends whatever conventions emerge from the evolutionary process and thus seems unable to fix the content of property conventions in the way libertarians want. These theorists need criteria to tell us why property conventions of a definite content are more desirable than others and why property conventions should predominate when they conflict with other kinds of conventions—for example, political conventions both within government and between government leaders and citizens. Without such criteria, libertarians merely take for granted that certain private property conventions are stable equilibria of the evolutionary process and that various kinds of conventions can in principle coexist in a sort of natural harmony within their respective separate spheres (e.g., market and government) if only ambitious political leaders can somehow be kept within their proper spheres. But the same general form of argument has been used by Michael Walzer in *Spheres of Justice* to defend a version of democratic socialism.

Finally, the Hindess volume, *Reactions to the Right,* offers a critical evaluation of contemporary British socialist thought, with a view to developing serious alternatives to New Right policy ideas. Three chapters together profess the need for a socialist alternative to both radical market socialism and radical democratic socialism—in short, a version of socialism that does not sacrifice traditional left-wing ideals of distributive justice to either unregulated market procedures or democratic procedures. Thus, Jim Tomlinson argues that more reliance on suitably regulated markets may be desirable, with the caveat that much work remains to be done on the design of a suitable "regulatory regime" to constrain powerful corporate bureaucracies (pp. 45–49); and Paul Hirst suggests that parliamentary reform is needed to bring about an "open corporatism" (p. 168). The four remaining chapters consider specific policy approaches in the areas of exchange rates, secondary education, welfare, and race relations. Much of the discussion is familiar in the American context and reminds the reader how deep an ideological gulf can separate moderates from radicals. For example, progressive conservatives and cautious liberals are unlikely to find helpful the radical notion of "institutional racism" discussed by Juliet Cook and Julian Clarke (p. 140). The notion implies that the white community, through its institutions, can be unjustly harming nonwhite minorities if institutional outcomes depart from some statistical measure of equality, even though nobody can point to any racist acts on the part of identifiable individuals.

Tulane University JONATHAN RILEY

The Political Philosophy of Michael Oakeshott. By Paul Franco. New Haven: Yale University Press, 1990. 277p. $30.00.

Most of the obituaries that followed the death of Michael Oakeshott in December 1990 were so inadequate that one must be especially grateful for Paul Franco's exhaustive and philosophically careful study. If one believed the *New York Times,* Oakeshott was a high-tory oracle, a "right-wing guru". If one did marginally better and gave credence to the *London Times* he was, though a philosopher who "stood fastidiously aside" from politics, nonetheless the "articulator" of the "real philosophical foundations of Mrs. Thatcher's policies." As Franco is careful to point out, Oakeshott had insisted as early as his first important Hobbes essay in *Scrutiny* (1935) that "every man, I suppose, has his political opinions. . . . But a political philosopher has something more: he has an analysis of political activity, a comprehensive view of the nature of political life, and it is this, and not his political opinions, which it is profitable for a later and different age to study" (p.). For Oakeshott, then, if political philosophy is to be worth more than the efforts of a fact-gathering *philosophe* such as Bentham ("as a thinker he is negligible"), it must "mediate between politics and eternity," must move from opinion to philosophy. Oakeshott made that clearest in a remarkable essay from 1938, "The Concept of a Philosophical Jurisprudence" in *Experience and Its Modes*: "Philosophical thought and knowledge is simply thought and knowledge without reservation or presupposition. . . . It should be thought of as something which happens at the end, when the concepts of science, or commonsense, or practical life are subjected to the revolutionary and dissolving criticism of being related to a universal context." (Franco helpfully rehabilitates this neglected essay, together with Oakeshott's other writings from the twenties and thirties.)

The stress on philosophy as "a universal, self-complete context" alone able to convert mere opinings into a "fully considered politics" that looks for its "reflection in eternity" suggests (as Franco sees) an Oakeshott rather different from the one made familiar in *Rationalism in Politics*. Indeed, one can wonder whether that brilliantly successful volume, with its insistence on the "sympathetic pursuit of intimations" within inherited historical traditions does not cast a misleading Brukean light on Oakeshott's vision of politics; for the chapter "Political Education" first annihilates two straw men (empirical ideological politics) in a way that reduces one to helpless laughter, then magnifies "our tradition of political behavior" in language redolent of Burke's *Reflections on the Revolution in France.* In much of *Rationalism in Politics,* one sees an Oakeshott wonderfully in command of a satirical idiom more narrowly English and less philosophical than elsewhere. For example "Freedom, like a recipe for game pie, is not a bright idea": the freedom of an Englishman is simply the availability of procedures such as habeas corpus.

Even those who doubted that *Rationalism in Politics* was as deep as it was effective could never have doubted that Oakeshott was, from the 1940s on, the supreme interpreter of Hobbes in this (or perhaps any) century. Franco well brings out the importance of Oakeshott's introduction to *Leviathan* (1946) and "The Moral Life in the Writings of Thomas Hobbes" (1960). With unerring insight, Oakeshott maintained that Hobbes's civil thought was the supreme expression of the philosophical tradition that viewed will and artifice as the heart of politics. Since Hobbes holds that "wills . . . make the essence of all covenants" (*Leviathan* 40) and that covenants alone generate sovereign legal authority, it seems that Oakeshott can scarcely have been wrong about Hobbes's voluntarism. Even if Oakeshott's own thought were to vanish, he would remain consequential as the most intelligent illuminator of "the sole masterpiece of political philosophy written in the English language."

Franco quite properly reserves his main interpretive efforts for Oakeshott's magnum opus, *On Human Conduct* (1975). That book is divided into three parts: (1) a compressed, dense, difficult account of human agency, intelligent self-determination, and self-enactment; (2) an account of the "civil condition," or *respublica* (above all authoritative *lex*), which citizens are to subscribe to in their pursuit of self-chosen satisfactions, with politics restricted to maintaining and occasionally rethinking *lex* (rather than indulging in a Baconian transformation of the world through joint enterprise); (3) an account of the advent of every significant moral disposition that has appeared in Europe since the fall of the Roman Empire, notable for distinguishing between the state as a *societas* of authoritative rules subscribed to by citizens in their chosen self-enactments and the state as a *universitas*, an "enterprise association" ruled by "teleocratic" managers in pursuit of a "substantive common purpose." In pages of great brilliance and insight (well treated by Franco), Oakeshott urges that the state as *societas* has been most adequately theorized by Aristotle, Hobbes, and Hegel (the half-dozen pages on the *Philosophy of Right* are incomparable) and is clearly contemptuous of the "servile" notion of the state as a "warm" *universitas* providing a "social end" to anti-individuals who have no self-chosen purposes. (Here, Aquinas, Calvin, Bossuet, Rousseau, and Marx are lumped as partisans of teleocratic "enterprise management"; and Montaigne is celebrated as the greatest understander of "the human condition as an adventure in personal self-enactment".) If *On Human Conduct* now receives a more sympathetic reading than it got in the 1970s, that will be in part because Franco shows how extraordinary a work it really is.

If, indeed, there is anything to complain of in this excellent piece of scholarly interpretation, it is that Franco tries a little too hard (mainly at the end) to relate Oakeshott to contemporary debates over liberalism and communitarianism, attempting contrasts with Rawls, Rorty, Dworkin, Sandel, and so on. But Oakeshott's real rapport is with Hobbes, Montaigne, Montesquieu, and (above all) Hegel—as Franco himself proves. If Oakeshott used the vocabulary of Roman jurisprudence (*respublica, lex, societas, universitas*), that was in part to mediate between his own politics and "eternity"—to make himself a part of the long philosophical tradition that he memorably interpreted and faithfully sustained.

University of Wisconsin, Madison PATRICK RILEY

Resurrecting Marx: The Analytical Marxists on Freedom, Exploitation, and Justice. By David Gordon. New Brunswick: Transaction Books, 1990. 155p. $29.95 cloth, $16.95 paper.

Political Identity: Thinking through Marx. By Robert Meister. Cambridge: Basil Blackwell, 1990. 426p. $44.95 cloth, $17.95 paper.

These two books offer radically alternative ways of making sense of Marxism. David Gordon dismisses Marxism as a viable theoretical position in any of its many possible formulations. Robert Meister creatively considers how Marx's work itself can be useful in making sense of contemporary politics, economics and philosophy and ultimately comes to the conclusion "that the Marxian moment is now" (p. 348).

Gordon claims that economists and philosophers in the 1950s and 1960s considered Marxism an "outmoded relic." The "analytic Marxists" (his focus is on G. A. Cohen, Jon Elster, and John Roemer) tried to resurrect Marxism using the tools of analytical philosophy. He presumably uses the same tools to examine their work. The book, though superficially sophisticated, is based on a rather crude argument. Capitalism is not exploitative, Robert Nozick is right, and an abstract model of laissez-faire capitalism can correctly define justice. The analytical Marxists cannot refute the "*prima facie* argument for the justice of capitalism," namely, that "*other things being equal*, . . . a just economic system is one in which people are free to make any economic arrangements they wish, so long as they do not coerce others" (p. 33). Therefore analytical Marxism is "a complete failure" (p. 144). This is hardly the "neutral" approach to the issue of capitalism we were promised (p. 33). Without serious empirical evidence there is no reason to accept his claim that "laissez faire capitalism has just this desirable feature [justice] while socialism, as usually interpreted, does not" (p. 77).

Meister asks how we might understand the relevance of Marx's work for contemporary criticism of existing institutions. His main theme is that it is essential to understand Marx as a critical theorist responding to the political and intellectual climate in which he worked. He makes a compelling case that the present situation is remarkably similar to that in Western Europe following the revolutionary upheavals of 1848 and that Marx's critical method can be used now in much the same way as it was then.

Gordon praises the "analytical Marxists" because they ignore or reject anything that even vaguely resembles "dialectics" or the "bizarre and hermetic terminology" (p. 19) of those who would attempt to make sense of Marxism as a response to Hegelian thought. Meister argues for renewal of Marxian critique as a response to the Hegelian character of modern thinkers ranging from Francis Fukuyama and his "mentor" Allan Bloom (p. 345) through Samuel Huntington and Sheldon Wolin to academics representing the New Left. Meister's explication of Hegel should be required reading for those tempted to take Gordon's view of Hegel's relevance to modern thought.

The differences between the two approaches are clearest in their treatment of Marx's economic theories. Gordon is satisfied to reject the "labor theory of value" largely because it offers no useful account of the prices of commodities. He deigns to note neither that Marx adopted the labor theory from Locke, Smith, and Ricardo nor the implications of this fact. Meister, in his normal scholarly manner, engages in an extensive discussion of what Marx actually said about economics and how this related to his critique of the classical political economists. He uses a novel reading of *Capital* to reject some important elements of Marx's substantive economic views. He shows how Marx's basic method of economic critique is tied to his critique of ideology and politics and concludes that even though Marx made some important substantive errors in his economic thought, his basic method is extremely useful in examining contemporary economic systems and economic theory.

Meister rejects all simplistic notions of what Marx had to say about politics. Gordon hardly addresses the subject except in his simplistic version of a kind of rational choice notion about justice. Meister argues that Marx's view of the relation of classes and political interests was a complex one that attempted to make sense of why "some historical differences between groups become politically salient, while others, apparently as great, do not" (p. 153). He suggests that the important political question is to decide "when and whether to support ideologically progressive demands of those victims of class exploitation, and how to regard demands of those victims that are not yet ideologically progressive" (p. 153). This analysis is far superior to dogmatic notions about class relationships or to much contemporary discussion of how groups other than classes make sense of progressive politics. His critique of feminist theory throughout the work is especially compelling in this respect. Meister argues that those who are to be guided by Marx's thought must exorcise the ghosts of Rousseau and Hegel and avoid the "revolutionary mystique" of much Marxist analysis, emphasizing what he sees as Marx's more mature political views that developed in the nonrevolutionary context that followed 1848.

Progressive students of politics and political activists who think they can find guidance in Marx must consider Meister's work. They will find much of his analysis at least as critical of the Left as it is of the Right. Those who reject this whole political perspective will nevertheless find much of interest in, for example, Meister's discussion of the concepts of pluralism and corporativism and his lengthy discussion of Tocqueville. Gordon's work may be of value to students of analytical philosophy interested in Marx and to those looking for a model of libertarian polemic through which to eliminate even the last vestiges of socialist theory. Students of political thought, whatever their own political perspective, should read Meister as an example of serious analysis; Gordon can be passed over with little loss.

California State University, Fullerton BRUCE E. WRIGHT

Simians, Cyborgs, and Women: The Reinvention of Nature. By Donna J. Haraway. New York: Routledge, 1991. 287p. $55.00 cloth, $16.95 paper.

Donna Haraway is an original and exciting voice in feminist theory. This book of essays, the author notes, marks her evolution from a "US socialist–feminist, white female, hominid biologist" to a "multiply marked cyborg feminist" (p. 1). Yet I am struck by the consistency over time of the two passions that have motivated her work: understanding science and devoting politically motivated attention to difference–multiplicity (whether produced along the lines of race, gender, class, or sexuality). Both these discourses, she argues, must be understood as social constructions. Her dense, allusive, and playful prose moves back and forth among discussions of biology, primate studies, philosophy of science, literary criticism, science fiction, and her own mythmaking. For social scientists, Haraway's efforts should make it far more difficult to maintain an image of the natural sciences as more objective than the social sciences could ever be.

Haraway, from the earliest essay to the most recent, attempts to denaturalize science. The first three essays in the book focus on theories in biology. In the first, "Animal Sociology and a Natural Economy of Body Politics," she argues that in primate studies one finds a "naturalization of the patriarchal division of authority in the body politic" in such a way that the fallacies of any claim to objectivity are exposed (p. 12). At the same time, Haraway refuses the distinction between bad (ideologically marked) science and good (objective) science. Science, she argues is neither discovery nor invention but, rather, a process through which we "both learn about and create nature and ourselves" (p. 42). She refuses the split between nature and culture and insists that the creation of ourselves is bodily, as well as social.

The other two essays in this section explore similar themes through different literatures. "The Past as Contested Zone" argues that it mattered that some of the scientists who worked on evolution by studying primates were women. They rewrote the stories men had put forward about human evolution and shifted the arguments from man the hunter to woman the gatherer. "The Biological Enterprise" examines how the images of the machine and the market have centrally organized theories of biology. Once again, Haraway shows science to be permeated with, and structured by, the social relations of a particular time and place. But once again, Haraway insists that there is no clear distinction between "objective science and abusive ideology" (p. 67).

Part 2 pushes further the claim that what we regard as science is deeply marked by social relations; and in focusing on the notion of "contested readings," Haraway calls attention to issues of power in both science and social life and to how power structures what will count as legitimate readings or even "real" experience. "In the Beginning Was the Word" and "The Contest for Primate Nature" insist on the fictive character of science as the telling of important public stories that rest on the process of production and reproduction of human life. But because science is not a mechanical reflection of ideology, Haraway is concerned to trace the development of the new stories told by women primatologists, point to questions about who saw what in the field, and document the destabilization of "facts." "Reading Buchi Emecheta" (a Nigerian novelist) grows from Haraway's experience as an interdisciplinary women's studies scholar. Yet her argument here resembles that of the first two essays. Now experience, as well as science, must be recognized as an intentional artifactual construction. Experience—in particular the experience of women—is not immediately given but must be contested for.

All this lays the ground work for "Differential Politics for Inappropriated Others," which makes the most important, original, and indeed brilliant interdisciplinary contributions. In "Gender for a Marxist Dictionary," Haraway argues that an adequate feminist theory must be a theory of racial difference in specific historical conditions and calls for the development of theories of difference whose geometries can break out of binary oppositions. One such dictotomy important for Haraway is the sex/gender (or nature/culture) opposition. While Haraway recognizes the strategic usefulness of the sex/gender distinction, and how gender was developed as a category to problematize what was to count as a "woman," she argues that the categories of sex, flesh, body, biology, race, and nature must be historicized.

In her now classic "Cyborg Manifesto" Haraway argues that boundaries between human and animal, organism and machine, and physical and nonphysical machine have all become ambiguous in the late twentieth century. In this world, feminists need to learn how to read webs of power and form new coalitions. She is beginning to tell her own feminist stories and to play with the possibilities for vision opened by science fiction in a postmodern world.

"Situated Knowledges" represents an effort to pull back a bit from the brink to which she moved in the previous essay. But once again, she shrinks from the dangers of an easy positioning, noting that while she wanted a strong tool for deconstructing science, the combination of radical social constructionism coupled with some versions of postmodernism could lead feminist theory into a "self-induced multiple personality disorder" (p. 186). The problem is how to cling simultaneously to an account of radical historical contingency for all knowledge claims and a commitment to faithful accounts of a "real" world. This is, indeed, an important problem for feminist theory. Haraway calls for partial, locatable, critical knowledges in which what had been the passive object of knowledge becomes an active agent—the world understood in the form of the Native American image of Coyote. The final essay, "The Biopolitics of Postmodern Bodies" returns to the themes of the more scientifically oriented pieces from earlier sections and once again subverts any logic of naturalization. "Science *is* culture," Haraway declares.

While Haraway was trained as a biologist, her work is genuinely interdisciplinary—in dialogue with feminist theorists from a wide variety of disciplines—and represents an important resource for political theorists. She undermines and subverts the image of natural science common among social scientists. In addition, her efforts to develop alternative geometries and strategies for reading social life in the late twentieth century are profoundly valuable additions to interdisciplinary scholarship in the social sciences.

University of Washington NANCY HARTSOCK

Voegelin on the Idea of Race: An Analysis of Modern European Racism. By Thomas W. Heilke. Baton Rouge: Louisiana State University Press, 1990. 161p. $22.50.

Throughout his career Eric Voegelin reminded us of the Platonic insight that political philosophy arises as an active resistance to the forces of human and social disorder that threaten the very possibility of the good life. In a nonideological era, such acts of resistance could draw upon the speculative traditions that were based upon the rational elucidation of an individual's experience of human existence-in-truth. Assuming the universality of such experience, available (according to Voegelin) in the undifferentiated insights of common sense, the topoi of discourse were established by the very structure of existence itself. Thus, meaningful debate and critique were possible because reason was rooted in the community of true existence; and that rootedness, in turn, provided both the ground for, and the rules of, serious discourse.

In Voegelin's analysis this possibility of truly rational debate has been seriously challenged by the emergence of political ideologies. Typically, ideological thought begins by first denying the authority of existence-in-truth and then proceeds by building its speculative structures upon an alternative or imaginary "reality" that is closed to the appeals of common sense. In this situation, debate becomes meaningless because speculation has been cut free from that discipline provided by a common and shared experiential base. Consequently, in an ideological age philosophical resistance to political disorder must take the form of an analysis of both the nature of the particular imaginary "reality" in question and the specific motivations accounting for its creation in the first place.

One particularly useful element of Thomas Heilke's *Voegelin on the Idea of Race* is that it presents to the reader a summary of Voegelin's earliest efforts to confront and thereby resist an important modern ideology. As a German living in Vienna from 1910 to 1938, Voegelin experienced firsthand the reality of an emerging political community in the process of ordering itself according to the fundamentally irrational principles of a racist ideology. In an effort to understand this phenomena, he published two texts in 1933 that have yet to be translated into English: *Rasse und Staat* and *Die Rassenidee in der Geistesgeschichte von Ray bis Carus.* Their publication led to Voegelin's being regarded as an enemy by the Nazis and his eventual flight from Austria following the German occupation in 1938.

At one level, Heilke's account is a fairly straightforward effort to summarize the contents of the two texts. He divides Voegelin's argument into three sections. The first, developed within *Rassenidee,* is essentially a historical account of the idea of race up through the end of the nineteenth century. Of specific importance to Voegelin is the perceived transformation of the race idea from an essentially transcendental understanding of life in general and human life in particular to a fundamentally imminent perspective on both.

The second and third elements of Voegelin's arguments are contained within the single text of *Rasse und Staat.* First, Voegelin sought to explore the status of the scientific claims that accompanied the emergence of modern racial ideology. In doing so, he paid particular attention to the tendency of race theorists to be epistemologically naive about what it was they were doing and thus substitute "speculative constructs" for the real units of theoretical analysis. Second, Voegelin explored the role of race ideas in informing and sustaining the shared web of meanings, beliefs, and expectations that partially constituted the European political communities of his day. The efficacy of such ideas can be partly understood in terms of the felt need for a concept to help order and explain the variety of human types that was becoming increasingly visible as a result of the expansion of Europe's historical and geographical horizons in the nineteenth century.

In addition to seeking to introduce the English reader to an important theme in Voegelin's earliest works and, in doing so, to provide background for a better understanding of his mature work, Heilke also intends to extract "a series of analytical tools from Voegelin's early though that aid in understanding political phenomena in genera" (p. x). Here, however, the volume is less successful. Heilke does, indeed, discuss such Voegelinean themes as archetype, ideas, experience, and mystery—but in a somewhat elementary manner. Part of this may be due to the fact that Heilke is primarily concerned with those analytical tools insofar as they explain the meaning of the text with which he is working, rather than as focal objects of his own analysis. At the same time, however, the elementary nature of this discussion may also be due to the fact that these analytical concepts were still not fully developed in Voegelin's own thinking. Voegelin's late realization that in order to understand an ideology one should move beyond a review of its statements to an analysis both of its "imaginary reality" and of the motives that explain its creation is anticipated, to some extent, in these early works. (See Voegelin's discussion of the essentially mysterious quality of our human unity and his analysis of Schelling's concept of *Angst*.) Yet it does not receive the focused attention that would come to characterize Voegelin's mature work on political order and human consciousness.

Loyola University, Chicago JAMES L. WISER

Can Modernity Survive? By Agnes Heller. Berkeley: University of California Press, 1990. 177p. $25.00.

Agnes Heller's latest book is a collection of essays organized around the theme of modernity and its prospects. It is an elegant, erudite book, written with Heller's usual clarity and cogency, addressing issues at once philosophical, political, and ethical. Heller—philosopher, prolific member of the Budapest School, and celebrated *intellectuelle engagée*—is a committed modernist and writes with the urgency of an unreconstructed partisan seeking to convince her readers of the risks to modernity and the enormous benefits, primarily political, that come from its actualization. Her essays have modernity as their subject; and they are, in themselves, the kind of urbane, carefully reasoned, politically aware statements of a decidedly modern theorist.

As the title suggests, *Can Modernity Survive?* focuses on a the conditions and possibilities of modern social and political arrangements and their future and advocates their preservation and actualization against threats from within, that is, from the contingency of modernity itself. According to Heller, modernity is not a "totality" but a framework to house a multitude of social orders and ways of life. Hence, modernity does not advocate one "supreme" way of life or set of values. Instead, "modernity does not merely tolerate, it positively cultivates difference." Its hallmark is difference and diversity—in a word, *freedom* to "transform one's contingency into one's destiny." The concretization of freedom is modernity's project, Heller argues, and its precondition: "Freedom is the central modern value and the interpretation and determination of freedom is what modern politics is all about." The question of modernity's survival, then, is primarily a political question, an inquiry into the prospects for freedom.

The same contingency which underlies the modern experiment also threatens to destabilize modernity's accomplishments. As Heller notes, the more we permit diversity, the more people begin to live uniformly. The more industrialization spreads, the more working conditions resemble one another. The "symmetrical reciprocity" of modern social relations results in conformity and conditions of mass culture. Modernity's freedom is a camera obscura, Marx wrote, subverting the moral freedom of the Kantian individual (which Heller clearly prizes). Heller hopes to counter the " 'death wish' at the heart of modernity" by outlining the lessons learned in the Western experiment with modernity and urging their cultivation.

She turns initially, in "Hermeneutics of Social Science," to the constitutive role of the social sciences in the formation of self-knowledge. The reader is reminded of the three functions of an informative social science: to create meaning, not primarily to solve problems; to establish true knowledge, not truth; and to provide understanding, which requires translation back into the language of the subject. A humane, modest activity, social-scientific inquiry, in Heller's view, is a moral and political enterprise. The ethical component of political and philosophical discourse is again strikingly endorsed in "What Is Practical Reason?," where Heller examines the political responsibility of the philosopher—in particular, the moral status of Heidegger, the man and philosopher, and the political status of his philosophy. In other essays, Heller examines the concept of the political (a distinctly modern concept, she maintains), the possibility for an emotionally fulfilling life, the priority of freedom over happiness, and the importance of political rights.

Throughout it all, one looks for criticisms of modernity or discussions of postmodern commentaries on modernity: its Eurocentric bias, its reliance on the state, its support of technology, its destruction of "subaltern" cultures. But for Heller, the criticism seems to be that the modern was not instituted thoroughly enough. Moreover, the postmodern is not a critique or a commentary on the modern but the culmination of the modern: "Postmodern is not what follows after the modern age, but what follows after the unfolding of modernity. Once the main categories of modernity have emerged, the historical tempo slows down and the real work on the possibilities begins." The possibilities of modernity are, it seems, just beginning with the emergence of the postmodern. With postmodernity, we are promised more of the same, only better. Thus, the answer to "Can modernity survive?" is a guarded and qualified *yes*. Modernity can survive if we improve on it.

Macalester College LESLIE J. VAUGHAN

History and Modernity in the Thought of Thomas Hobbes. By Robert P. Kraynak. Ithaca: Cornell University Press, 1990. 224p. $28.50.

When once asked what he understood himself to be, Leo Strauss is reputed to have answered, "an historian." Whatever the valence of Strauss's use of the term, Kraynak here has offered a reading of Hobbes that is in certain respects sympathetic to Strauss and provides the long-overdue service of assessing Hobbes's theory of civil history and its bearing upon the rather abstract and ahistorical political theory with which readers of *Leviathan* are familiar.

Notwithstanding the precariousness of life in the hypothetical state of nature, Kraynak convincingly argues that Hobbes's extensive reading of civil history led him to the conclusion that the struggle for survival that marked the early stages of civil history had been supplanted by a struggle of opinion—an age of "doctrinal politics—that was far more dangerous (chap. 1). Echoing Rousseau, Hobbes would have it that vanity, the wish to think highly of oneself—in short, the very thing that separates us from the beasts—makes human struggle so unlike the struggle in the animal world. Contra Marx, *human* struggle in the civil, doctrinal age is not *really* over resources.

The Hobbesian project, synonymous here with the project of modernity, purports to offer the only antidote for the intellectual vanity at the root of doctrinal politics—to deny the authority of all vain and ambitious men who claim wisdom for themselves. Hobbes's question—"Is it possible to overcome the passion of intellectual vanity and the historical tendency of the human mind to trust in the words and wisdom of authority?" (p. 96)—was answered with a resounding *yes*. And his success in undermining such authority marks the beginning of the illegitimacy of the modern age.

Kraynak offers a careful reading of Hobbes interwoven, as the book proceeds, with an Aristotelian counterpoint at times overtly incredulous at the audacity of Hobbes's project and, by extension, the modern project. Chapter 5 is particularly unforgiving of Hobbes, exposing, as it does, his supposition about the self-reliant mind and its method of introspection. Dialectic gives way to introspection. The search for reality gives way to an inquiry into states of mind and their cause. "Still protected," however, "by the philosophic and academic traditions of the West, [which hold] that a science of nature and natural justice is possible" (p. 197), the modern mind does not yet become the "existential mind." However, having dissevered itself from the tradition, the latter is but a short step away. The self-reliant mind is a fiction that can end only in the subjective morass of solipsism, notwithstanding its initial posture, Kraynak seems to intimate.

All authority now suspect, sovereign power must serve the function of arbiter (p. 171), of arresting vanity and the claims of authoritative wisdom. This antidote to doctrinal politics is purchased, however, at a high price. It violates the classical insight that "the 'cave' of the political community—its authoritative opinions and traditions fashioned by the poets, priests, and legislators—could be transcended only by the privileged few philosophers" (p. 188). Now, in effect, not only are the few who can bear the absence of authoritative opinion allowed to do so (the ancient insight) but everyone is forced to live in accordance with this verity. Indeed, the structure of political life, the need for a sovereign who is merely an arbiter in a world bereft of authority, reflects it.

The conventional wisdom about Hobbes, of course, is that he (perhaps along with Machiavelli) inaugurates modern political realism. Yet Kraynak would turn this view on its head. Because Hobbes disregards "the powerful human tendency" to trust in authority (p. 130), he is *less* realistic than were Aristotle and the scholastics. They at least knew that the human mind is not self-reliant, that it must trust in the authority of others. Because this tendency is a permanent feature of the human condition, Kraynak seems to intimate, Hobbes's edifice, the modern edifice, is built upon a foundation of sand.

Several objections do come to mind. First, it is to be wondered how the intellectual vanity Hobbes thought so necessary to combat may be overcome by a kind of resolute acceptance of the tendency to trust authority. Only human beings, Hobbes reminds us, have the privilege of absurdity—the privilege of going awry that possessing language bestows. Whatever may be said of the theoretical inadequacy of Hobbes's formulations, the suspicion against intellectual vanity has been burned into the modern mind on his account, and this has been for the good. Would Kraynak want us to return to the scholastic debates that had human authority on their side, if nothing else? To be sure, Kraynak is not oblivious to this problem and so countenances a kind of humility at the end of his book (p. 216). But the question remains whether the temptation of vanity can be vitiated without, as it were, the necessary illusion that "knowledge" based upon the authority of the one propounding ought rather to be called *belief*. Human beings are prideful creatures, as Hobbes over and over again reminds us. The pretensions of wisdom ought to raise suspicion not because wisdom does not exist but because while it can raise us almost to the level of the angels (following Locke), it can also lower us to a level below the beasts.

Second, the question needs to be posed whether *civil* history is the proper horizon within which to view Hobbes's thinking. History is crucial—this is Kraynak's important contribution; but Hobbes places civil history within the larger horizon of *biblical* history. Reason can know the former; only revelation illuminates the latter. Reason may conclude that there is a need for an antidote to doctrinal politics; but revelation confirms that the right pattern of political association is the Mosaic covenant wherein the sovereign has the right to command obedience and the right to interpret—attributes that the Leviathan, not incidentally, possesses. Reason and revelation, as Hobbes insists, do not conflict. By focusing only upon reason and the civil horizon of history within its purview, only half of the picture emerges. Hence, the embarrassment of the attributes of the Leviathan cannot be satisfactorily accounted for.

In other words, like Strauss, Kraynak understands Hobbes's edifice to be operating without superhuman support (p. 179), without revelation. The historian in Kraynak has made an important contribution to our understanding of Hobbes. The criticism to be made is that in the end, he is not enough of an historian, does not accord biblical history its due. Hobbes's science may be fully illuminated without recurring to biblical history, but his politics may not.

George Washington University JOSHUA MITCHELL

The Political Philosophy of Montaigne. By David Lewis Schaefer. Ithaca: Cornell University Press, 1990. 407p. $41.50 cloth, $13.95 paper.

All praise to the scholar who in his youth resists the enduring attractions and current temptations to publish and persists with his studies and rewritings until they and he reach a maturity from which the world of serious inquirers will benefit! This volume is the fruit of David Schaefer's long consideration of Montaigne. For years, in essays, panels, and other scholarly endeavors, he has argued for a serious consideration of the thought of Montaigne—its consistency, its power, and its pertinence to the foundation of the modern liberal order (including the state), to the academic political science practiced in the liberal university that is the image of that state, and to that suburb of the university called the French department. Schaefer's book presents to Montaigne studies a Montaigne that is more political and teacherly than private and self-savoring. To political science it thrusts forward a Montaigne who is the cunning consistent founder of liberal modernity. To the diverse and divided university of the present day, it may disclose a thinker whose thoughts are too unified to fit in any one department and too significant to be dubbed interdisciplinary.

It could not be these good things without being both exceedingly detailed and somewhat comprehensive. Since Montaigne is divers, wavelike, and savory to the point of seeming casual, inconsistent, and even self-indulgent, the majority of the book is devoted to detailed, careful, even suspicious readings of important chapters from the *Essais* (esp. 1.23, 31; 2.11–12; 3.1, 4 & 9—with by far the greatest attention paid to 2.12. "The Apology for Raymond Sebond," which previous generations took to be central to Montaigne's achievement and understood to be skeptical (e.g. Sainte-Beuve). Schaefer's fine readings should be read along with the chapters, slowly. Thus, the book includes a careful treading of Montaigne's footpaths, by stile and gate, and an attempt to see his whole design, as if in one high-altitude topographic survey. In the latter, which concludes the readings, Schaefer emphasizes the "intermediary" service of Montaigne between founder Machiavelli and his liberal tamers (Locke, etc.). Here Schaefer hangs from the lion and fox of Machiavelli studies, Leo Strauss and Harvey Mansfield.

Naturally, such an ambitious book and its achievements raise questions for the reader, the author, and the rereader (for the book is worthy of such). Granting for a moment that Montaigne teaches covertly, why ignore the amiable face of the teacher? Isn't the secret of the depth in the surface as well? And granting that what Montaigne teaches tends to found something like the liberal order (which I am partly disposed to believe by my own researches), did not Montaigne teach it, as well, by the liberal, free, gentle self he presents the unsuspecting reader? When aiming at one goal, isn't it best to arrive by divers means? By argument and by example? In any case, can there be a good book without examples or a great book on human things without some great examples?

Is Montaigne such? Of all authors of great books, he is perhaps the one readers would most like to meet. Is he trustworthy, as well? Who is Montaigne? The characterization of Montaigne as Machiavelli's student (and esteemer of Tiberius) implies that he might not be one who has suffered. Certainly, Pascal might agree: not recognizing suffering or joy, the liberal man is overcome by the one and cannot rise to the other. Tocqueville might add that without the virtues it disdains to inculcate, liberalism will give way to tyranny. What would Montaigne reply? When he acknowledges himself no Socrates, (let alone Christ), Montaigne certainly accords with Machiavelli's lowering of the standards—but not wholeheartedly, for Socrates (at least part of him) shines in the *Essais* as never in the *Prince* or *Discourses*. Could it be that Montaigne differs quite a bit from the unarmed Machiavelli whose single bite at once maddens the clever and arms them to the teeth?

It is the merit of this book that it provokes such questions (and many others). This worthy fruit of the author's long consideration of Montaigne, along with the author's honest acknowledgment that many things baffle him utterly, or elude his firm grasp is the strongest reason to look forward to his further consideration of Montaigne—the generous founder of an order that cannot acknowledge him but whose citizens will always benefit from reading him thoughtfully. David Schaefer is a member of the liberal university who makes it surpass the liberal order in richness, nobility, and intellectual honesty—which it must do if that order is to retain its characteristic virtue, liberty.

Dartmouth College MICHAEL PLATT

The Limits of Government: An Essay on the Public Goods Argument. By David Schmidtz. Boulder: Westview, 1990. 197p. $38.50 cloth, $12.95 paper.

Why must governments exist? The common philosophical response to this question is some variant on the public goods argument. A public good, approximately, "is a good that can be produced only by collective action, but [whose] production benefits people regardless of whether they join in the collective effort" (p. 1). All profit from passable streets and secure borders; government exists to ensure that all likewise pay. If streets and the common defense were left to the market, they would probably not be produced. If their funding depended on voluntary action, some would presumably contribute, but others would take the free ride. Thus, coercion compelling participation in the production of public goods is essential—hence, the state. It is "not that people consent to government, but rather that government does something that needs doing" (p. 10).

David Schmidtz has produced a literate and at times charming essay on the (rather deep) limits of the public goods argument. Successive chapters deal with the legitimacy of property and property rights and the famous Lockean Proviso, the emergence of the right to punish, various versions of the prisoner's dilemma as applied to the public goods argument, and a theory of morality (enshrining reciprocity as the highest moral virtue) in a cooperative society.

Coercion is required for the production of public goods—hence, the state is necessary—only under two conditions: (1) if the good is nonexclusionary (all may partake whether they contributed to its production or not), then some would choose not to contribute because they can enjoy the good for free; (2) some essential goods may be hopelessly underfunded and thus never produced if their production relied on voluntary contributions. This is known as the *assurance* problem. If a noncoercive solution to the free-rider and assurance problems could be found, then the public goods argument in behalf of the state would fail. In the language of game theory (and of this book), this amounts to defining a prisoner's dilemma where cooperation is a dominant strategy.

Schmidtz argues that assurance contracts can solve the assurance problem. In essence, the assurance contract is a money-back guarantee. Contributors get their donations back if the total donations are insufficient to provide the good. Evidence from experimental economics tends to show that voluntary contributions to the collective good increase under assurance contract conditions (chap. 6). The free-rider problem is less easily solved, owing mainly to the difficulty of distinguishing between free-riders and "honest holdouts" (people who do not value, or would not profit from, a particular public good): "The real point of coercion is to force even honest holdouts to pay" (p. 84). And therein lies the legitimation rub: By what right does government expropriate the property of honest holdouts or (what amounts to the same thing) force people to buy that which they do not value?

Thus, the public goods argument is unsound: "Its descriptive premise is that coercive public goods production makes each person better off from the person's own perspective. The argument's normative premise is that when coercion does this, coercion is permissible. The descriptive premise is, by and large, false"—precisely because honest holdouts in fact exist (p. 84).

The problem with the coercive production of public goods, of course, is that overproduction is stimulated (as opposed to the underproduction characteristic of a free-market situation). Coercers can (and do) finance their own projects "without regard to whether the income they are spending is their own" (p. 89). There is evidently no limit to how costly overproduction can be, at least not if the recent history of public spending is taken seriously; but there is a definite limit to how costly underproduction can be, "for the worst that can happen is that the good is not produced at all" (p. 90).

The concluding chapter, "The Morality of a Cooperative Society," is an effort to develop a more adequate normative basis for the public goods argument. Not belaboring the details, a reworked version of the public goods argument "can justify more than a minimal state, perhaps, but not a great deal more. The justification of big government requires a different kind of argument" (p. 160).

The public goods argument lies at the heart of much contemporary political theory. *The Limits of Government* is a sustained and persuasive attack and is thus required reading for theorists throughout the social sciences.

Tulane University JAMES D. WRIGHT

Reason and Democracy. By Thomas A. Spragens, Jr. Durham: Duke University Press, 1990. 281p. $39.50 cloth, $17.95 paper.

In *The Irony of Liberal Reason* (1981), Thomas Spragens charted the various miscalculations and subsequent disappointments that attended the early marriage of liberal hopes to the epistemological projects of Descartes and Locke. The irony was that because liberals expected too much of reason, they were forced to settle for less (e.g., instrumental rationality and value noncognitivism). Lacking any philosophical resources for countering the equation of liberal reason with a calculative ideal, liberalism seemed well suited to one or another form of power politics but ill suited to any more generous vision of politics. *Reason and Democracy* shows us that we can and should do better. It is an argument presented in two parts. The first part briefly retells how postpositivist developments in the philosophy of science and language enable us to recapture the pre-Weberian belief that rationality can be used as the standard of a good society. The second and larger part of *Reason and Democracy* aims at showing us how the values and ideals of human reason conceived as a certain type of praxis provide us with a clearer statement of the importance and meaning of liberal democracy.

Spragens's account of how reason came to be reduced to a model of means–ends calculation and his critique of that model will be familiar to those who have either read his earlier work or been following the arguments for more contextual (practical/communicative/discursive) conceptions of reason. As Spragens reads these developments, they essentially involve a shift away from the notion of reason as a logic and technique of neutral observation and toward an appreciation of reason as a formative process of inquiry that constitutes an act of "collective prudence" or judgment. Recognizing that rationality itself has a practical dimension "enlarges the range of rationality, jettisons demands for and claims of certitude as a hallmark of reason, shifts the focus of norms of rationality from content to process, and substitutes a paradigm of a communal dialogue for the paradigm of a logical soliloquy" (p. 110). Publicity, communication, fairness, toleration, rights of participation, duties of veracity and responsibility, the discipline of reasoned discourse: each plays a role in science understood as a rational enterprise. The bulk of *Reason and Democracy* deploys these norms of rational practice in order to characterize and justify liberal democracy. It is Spragens's contention that democracy is at its best when it most resembles human reason at its best: "If we had to choose a single normative standard for the understanding and evaluation of liberal democratic political systems, the one that would get us the farthest would be neither liberty, nor fairness, nor neutrality, nor utility, nor pluralist bargaining, but would instead be the ideal of rational practice" (p. 255).

This contextual turn, in which standards of good practice are uncovered in some activity when it is done well is a familiar pragmatic move in contemporary political theory. Charles Anderson's *Pragmatic Liberalism* (1990) argues that pragmatism gives liberalism a method and that liberalism gives pragmatism purpose. Benjamin Barber's *Strong Democracy* (1984) echoes John Dewey in its thesis that political rationality is best understood as a type of social intelligence that is expressed in democratic processes of common deliberation. *Reason and Democracy* is consistent with these and similar works, but it is in some ways a more ambitious book. Spragens uses the ideal of a rational society to measure the contributions of liberal, communitarian, republican, and other theories to the project of democratic rationalism. There is room in his account of reason and democracy for liberal tolerance, a constrained common good, strong democratic talk, and a modified range of republican virtues. Since one is more used to theories that seek to defeat, rather than reconcile, competing doctrines, it is refreshing to follow Spragens as he looks for what is right in the work of various authors. But this generosity is not without its price. Controversies are suppressed, and differences smoothed out, in ways that permit Spragens to talk, for example, about the contributions of both liberals and communitarians to the ideal of a rational society. But if these differences reveal something about politics that we might otherwise miss, then we might not want a theory sufficiently capacious to accommodate both. At times, *Reason and Democracy* is too roomy. Conceived as a rational enterprise, the good society is "a complex form of social practice that protects rights but does not enshrine them in a vacuum, that protects and promotes equality but is not mindlessly egalitarian, that seeks justice but does not abstract it from the common good" (p. 238). Statements such as this leave many lines undrawn and what might count as a rational balance unclear.

Reason and Democracy could be read as a caution against the demand that reason reach as far as my last criticism implies that it should. Spragens might fairly remind his readers that it is precisely because liberals once mistakenly imagined that reason could make all of the difference in politics that we lost sight of the difference that it could make. Because it so clearly recaptures a political and moral role for reason in liberal societies, this book is a worthy sequel to Spragens's earlier work.

University of Florida ALFONSO J. DAMICO

The Idea of Political Theory: Reflections on the Self in Political Time and Place. By Tracy B. Strong. Notre Dame: University of Notre Dame Press, 1990. 196p. $28.95.

Any book with a title such as this one risks disappointment, in part owing to its big promises, in part owing to the difficulty of saying anything new on a topic so thoroughly surveyed and debated. But Strong's book is remarkably fresh; he reformulates traditional questions by looking at the ways they locate and constitute the self in the several kinds of worlds—aesthetic, moral, theoretical, political, and linguistic. This allows Strong to ask a traditional question—What is "the political" such that it could be an object of theory?—without providing a traditional answer. Traditional answers fail us not because they are "wrong" but because they take for granted precisely what we now experience as most problematic—a self, a personal identity that is intact and autonomous. "The political," Strong argues, is "that form of human activity in which the answer to the question 'who am I?' is also the answer to the question 'who are we?' " (p. 34). This way of putting the issue is perhaps deceptively simple. It is not, for example, to ask how we might reconcile differences to pursue common projects. Rather, it is to ask what kinds of problems map the terrain within which the "I" and we" are simultaneously constituted, an approach that owes more to Nietzsche and Wittgenstein than to Locke and Hobbes.

Most of Strong's book examines the implications of viewing "the political" in this way. Most importantly, a political terrain comes into being intersubjectively, through human interactions. This alone means that politics lacks qualities that we might wish it to have. It is imperfect, not entirely controllable, and does not admit of certainty. And yet we are always tempted to believe otherwise just because perfection, control, and certainty are necessary to self-identity. Yet if we give ourselves over to these temptations, we in effect cut ourselves off from the "we" and produce for ourselves a world with fewer possibilities and experiences.

One of the more remarkable aspects of Strong's book is that he shows how these insights are embedded in the canon. Augustine's view of the self as constituted by *will, vision,* and *memory,* for example, provides Strong with a way of analyzing three kinds of antipolitical temptations—topics of the middle three chapters. Vision is future-oriented, motivated by a desire for perfection and represented in its purest form by dramatic art (chap. 2). The will is present-oriented, seeking control over the "otherness" of the world and represented in science (chap. 3). Memory is oriented toward the past and tempted by the identities of "truth" that would, in effect, relieve the self of its dependence on contingencies and securing its identity in a permanent order (chap. 4).

Much of Strong's argument is devoted to these "temptations." On the one hand, they typify approaches to political theory that seek secure identities for the self. They do so, however, at the cost of obscuring political dimensions of existence, thus depriving humans of awareness of, and responsibility for, the political dimensions of their experience. On the other hand, politics can and must invoke these identities since "the political" draws on the problems and relations that these identities locate. It is precisely this overdetermination that makes politics a realm of contingency and resistent to metanarratives. These several kinds of relations exist in tension, yet each is necessary to constituting persons. This is why political theories that seek to ground themselves in any one of these realms become partial and lose their ability to provide a political articulation of the self.

If we are to understand political theory in a way that articulates our postmodern condition—one that Strong describes as an interrelated crisis of personal identity and political authority—then it must be about "boundary maintenance" between these necessary but irreconcilable realms (p. 2). Without holding these realms in tension, without allowing each its particular "mode of validity," the self is dispossessed of the situated identities through which it can become a political agent. Political theory's mode of validity consists in articulating these relationships while holding them in tension. One implication of this approach (to which Strong devotes chap. 5) is that the authority of political theory must be parochial, that is, relative to time and place. As to the problem of ethical relativism that seems to follow, Strong notes that the problem is conceived in a way that cannot produce a politically intelligible answer: universal commitments have a purchase on action only when selves are already constituted as moral agents by their relations to the "we." And this is a political, not an ethical, problem.

There are things one could quibble with in this book. For example, the catalog of "temptations" that constitute the self seems somewhat contrived or perhaps incomplete. For example, although Strong discusses religious, moral, and linguistic identity, they do not figure into the temporal framework of self-constitution that he borrows from Augustine. In addition, some of Strong's claims to originality depend on ungenerous readings of approaches to which he is indebted, such as phenomenology and hermeneutics. And sometimes the argument is awkward in ways that it need not be. For example, a little reading in Habermas—a master of arguments about distinctive modes of validity—would have saved a strained attempt to explain why political theory cannot be "true," but only "truthful" in articulating relations between "I" and "we" (pp. 118–20). The point is well taken but comes across badly because Strong uses *truthfulness* to cover two related but distinct modes of validity, namely, sincerity and legitimacy—in this way collapsing reflexive validity with respect to the self and acknowledged validity with respect to others.

Nonetheless, this is an important book. One has a sense that an approach to political theory that has been developing in bits and pieces for several generations (one might call it "critical postmodernism" even though Strong does not use the term) is now catching its stride. Strong's book speaks in a confident voice devoid of jargon. It puts forward positive, not merely critical, claims and visions. It is devoid of the whiny nostalgia for past paradigms—paradigms appropriate to other times and places—that mars so much contemporary political theory. And Strong convinces us that critically postmodern approaches can help us attend to a broader range of experiences, can induce a deeper spirit, and can provide more appropriate responses to suffering and contingency than their fading alternatives.

Georgetown University MARK WARREN

Moral Foundations of Constitutional Thought: Current Problems, Augustinian Prospects. By Graham Walker. Princeton: Princeton University Press, 1990. 189p. $25.00.

This book presents a normative critique of recent constitutional scholarship. It might best be described as a "second-level" analysis; that is, Walker has far more to say about those who write about the Constitution than about the Constitution itself. Indeed, he succeeds in writing a book about the Constitution without even mentioning a single decision by the Supreme Court or any other court.

The author's thesis is that constitutional scholarship has reached "normative impasses" grounded in the various philosophical foundations of the leading writers and that a fresh perspective is needed if we are to make any progress in "clarifying the fundamental and inescapable questions confronting constitutional thought" (p. 5.) Ambitiously but with considerable skill, Walker reaches back some fifteen hundred years to Augustine to provide this fresh approach.

The argument of the book is divided into two parts. First, the author explains the normative impasses of contemporary constitutional scholarship by dividing the field into four quadrants created by a horizontal axis separating teleologists from deontologists and then bisecting that axis with another that divides realists from conventionalists. A moral realist for Walker is one who "regards the good, whatever it may be, as a reality whose existence is independent of human artifice." Conventionalists affirm that the good is "a contingent human artifact" (p. 24).

With his quadrants established, Walker distributes prominent authors to their assigned places: Sotirios Barber and John Courtney Murray to teleological realism, Walter Berns to conventional teleology, William Rehnquist and Michael Perry to conventional deontology, and Ronald Dworkin and David A. J. Richards to realist deontology. One can only imagine the howls of protest and the shrieks of delight that will greet these placements. Surprises await the Senate Judiciary Committee when it discovers Laurence Tribe and Robert Bork sharing the rather cramped quarters of conventional deontology along with Rehnquist, Perry, Lief Carter, John Hart Ely, and Rogers Smith.

In what is surely the best part of the book, Walker develops a powerful argument against all conventionalists by showing how their position can ultimately be reduced to a "nihilist skepticism" that undermines serious moral analysis. One might question whether Walker has the right players in the right slots, but his argument against conventionalism is clear and unerring.

His attack on the realists is more problematic. Walker maintains that realists threaten to reduce law to morality. Because they have a substantive notion of the good, he maintains, they have no principled way to confine the sphere of law: "The deep logic of contemporary moral realism seems to suggest that law is coextensive with morality" (p. 59). All this is problematic because Walker fails to recognize the principled limitations on law in the jurisprudence of the prince of realists, Thomas Aquinas, and such twentieth century Thomists as the late John Courtney Murray, whose work Walker misconstrues.

Problematic or not, however, the criticism of the realists brings Walker to the second major point in his argument, namely, that Augustine comes to the rescue. Although Augustine is clearly a realist, his belief in man's fallen nature considerably tempers his expectation of improving human affairs by law. For Augustine the institution of government is itself the result of sin and, therefore, Augustine is in no danger of succumbing to what Walker sees as the realist's temptation to achieve authentic moral perfection by law.

Walker's presentation, serious and thoughtful throughout, is often persuasive and at times convincing. Yet this fine book is not without its shortcomings. The primary problem is Walker's somewhat confusing treatment of the theological basis of his argument. He tells us we need not share Augustine's faith in order to learn from his teachings on law but fails in his effort to disengage Augustinian political science from the faith of Augustine. As a matter of fact, Walker dedicates some 30 pages to a concise summary of Augustinian theology. With a polite bow to secular orthodoxy, Walker graciously grants the reader permission to pass over these pages; yet clearly, he hopes his readers will not avail themselves of his liberality.

The heart of the Augustinian relevance to Walker's argument is Augustine's clear-headed view of man's imperfection that keeps him from looking to law to perfect man morally. Walker assures the reader that for all his hard-headedness, Augustine is no cynic. This is surely correct, but only because of Augustine's faith. No serious Christian can be a cynic, and Augustine is a very serious Christian. In his unconvincing effort to uncouple Augustinian jurisprudence from the faith of the saint, Walker would give us *Hamlet* without the Prince of Denmark.

Virginia Polytechnic Institute JOHN A. ROHR

After Ideology: Recovering the Spiritual Foundations of Freedom. By David Walsh. San Francisco: Harper, 1990. 296p. $24.95.

As communism collapses and Marxism–Leninism is repudiated around the world, we have the sense that a distinct period of history has come to a close. Some may call it the age of totalitarianism and trace its course in the twentieth century through the rise and fall of fascism and communism. Others may look at the period from 1789 to 1989 and see the series of convulsions as a great experiment in political utopianism. Others may go even further and view the entire modern period from the Renaissance to the present as a completed event, with the twenty-first century marking the end of modernity and the dawn of the postmodern age. However the lines are drawn, one cannot deny the need for an explanation of these momentous events, as well as a moral compass to guide us through the next century or millenium.

While most political scientists might feel overwhelmed by such intellectual challenges, we can be grateful that David Walsh has not shied away from them. In his new book, Walsh attempts to understand the whole modern age as a dramatic spiritual crisis and to propose a solution. His thesis is that modernity is an age of ideology, driven by a misguided revolt against God; his solution is a carefully balanced synthesis of freedom and Christian faith.

Following the lead of Eric Voegelin, he traces the roots of modernity to a change that occurred within Christianity. Somewhere in the late Middle Ages, the "gnostic" spirit of blaming God for the fallenness of the world overtook certain Christian thinkers (beginning with Joachim of Fiore) and led them to seek the realization of the Kingdom of God in this world and eventually the reformation of the fallen world by purely human effort (pp. 99–116). From this spirit, the ideologies of the modern world were born. They are secularized or deformed versions of Christianity, driven by rebellion against God and the God-given limits of the natural order. They are captivated by visions of realizing heaven on earth (among other things the end of suffering and the perfectly harmonious society). Whatever form they may take—socialism, communism, fascism, genetic engineering, Freudian psychology—ideologies are intellectual constructs whose purpose is to transform human nature and conquer the natural world (pp. 10–12). In Walsh's judgment, the effort to remake the world through ideologies has been more of a nightmare than a pleasant dream; and the manifest failures have led to their demise in the present day.

While this analysis and critique is not new, Walsh develops it in a way that is fresh and in many respects profound. He explores the inner logic of the modern crisis of ideology by using what he calls the "existential" or "experiential" insight of four unusual figures: Camus, Dostoyevski, Solzhenitsyn, and Voegelin. Although the selection of these four may seem idiosyncratic, Walsh justifies his choices with the claim that each felt the modern spirit of revolt against God and overcame it through a kind of personal cleansing or purification of the soul—an actual religious conversion to Christianity or an acceptance of the need for God.

To support his claim, Walsh presents detailed analyses of the literary and philosophical works of each author. Camus is undoubtedly the most difficult to fit into this scheme; but Walsh makes a plausible case that Camus's novels *The Plague*, *The Rebel*, and *The Fall* are the works of a man anguished by the theological problem of innocent suffering and longing for an affirmation of the goodness of life—a quasi-Christian existentialism that yearns for redemptive grace without ever receiving it (pp. 142–48). With less straining, Walsh presents Dostoyevski and Solzhenitsyn as men who went through socialist phases in their lives and truly overcame the revolt against God. The most delicate question in both cases is whether they actually embraced the Christian faith—experienced real conversions—or merely equated the profound need for God in their lives with the real existence of God. To Walsh's credit, he presents the evidence as fairly as possible (pp. 148–84); but I am not sure how close one can get to understanding these tortured Russian souls! In the case of Voegelin, the situation is also complicated. Walsh quotes Voegelin's statement, "History is Christ written in large letters" but also notes Voegelin's "distinct coolness toward Christianity," implying that Voegelin's interest in Christ is that of an intellectual historian rather than that of a heartfelt believer (p. 201).

Despite the complicated nature of each thinker's relation to Christianity, the overcoming of the modern spirit of revolt in each case permits Walsh to draw lessons for modern politics. His conclusion is that the modern idea of freedom embodied in liberalism and constitutional government is salvageable from the wreckage of ideologies if it is established on Christian principles. In other words, liberalism is not just another ideology, because it has no inherent metaphysical grounding or messianic pretenses. Its notion of free individuals responsible for their actions does not require the doctrine of rights or even a metaphysical teaching about freedom of the will. Rather, Walsh argues, its best grounding is Christianity, properly understood.

But here is where Walsh fails to satisfy the critical reader. What does he mean by Christianity, and why is he so sure that it is compatible with liberal freedom? Walsh makes a very interesting case that Solzhenitsyn's Christianity is actually compatible with a certain kind of constitutional monarchy and local *zemstoy* or citizen assemblies (p. 232). On the other hand, Walsh fails to convince that Dostoyevski's faith and politics are compatible with western liberalism (pp. 211–12). Walsh's own position remains unclear, because he speaks repeatedly of "philosophic Christianity," which sounds like Thomism, and of a "Christian philosophy of existence," which could be either Augustinianism or the phenomenological Christianity of recent Catholic thinkers such as Henri de Lubac (pp. 4–7, 212–28). Since Walsh denies that he is in any way a reactionary, one may infer that he is not promoting a Thomist or Augustinian revival. The statement of his political program bears this out (pp. 272–78); for it closely resembles the position of that great nineteenth-century synthesizer of Christianity and liberal democracy, Alexis de Tocqueville. Thus, through a fascinating tour of four strange and mysterious human souls, Walsh has led us back to familiar institutions but with new insight about how they might be grounded in the experience of transcendent order.

Colgate University ROBERT P. KRAYNAK

The Politics of Being: The Political Thought of Martin Heidegger. By Richard Wolin. New York: Columbia University Press, 1990. 231p. $34.00.

As the intense debates—first in France, then in Germany—showed, the revelations about Heidegger's deep involvement with the Nazi movement published by (the often unreliable) Victor Farias and, more significantly, Hugo Ott, made it imperative that Heidegger's work be reread with a sharper eye for its political dimensions.

Richard Wolin, undoubtedly inspired by his earlier work on the French Heidegger debate, followed this imperative. The result is a very lucidly written and persuasively argued brief for the claim that Heidegger's involvement with National Socialism "was rooted in the innermost tendencies of his thought" (p. 66). Implicitly, Wolin thus argues against Richard Rorty's conviction that Heidegger's politics is explained purely in terms of his "pretty nasty character." Rorty's judgment that the relation between a writer's books and his life are purely contingent is shown simply not to stand up against overwhelming contrary evidence. In the stringency of his thought Wolin surpasses even Habermas. The latter, in an attempt to salvage the "autonomy of thought" of *Being and Time*, a book that had enthralled him in his own youth, claims that after 1929 a "process of ideologizing" or *Verweltanschaulichung* of Heidegger's pure theory destroyed its initial integrity (*Texte und Kontexte* [1991], pp. 58, 60). Wolin disputes Habermas' contention that Heidegger is wrenched by an internal struggle between pure philosophy and the popular German conservative ideology of cultural decline. Instead, Wolin can show that from the start, Heidegger's most fundamental philosophical premisses issued necessarily in his peculiar reading of National Socialism. Consequently, Wolin's basic argument generally is in agreement with the writings of Otto Poeggeler, Alexander Schwan, and Karl Loewith.

But Wolin's argumentation is very much his own and offers some new insights. He begins with two facts: that Heidegger himself claimed an "essential" relationship between his philosophy and National Socialism and that this was true despite the absence in his work of an articulated practical philosophy. The answer lies in Heidegger's philosophical attempt to eliminate altogether the traditional division between theoretical and practical reason. Heidegger accomplishes this, however, by essentially equating basic ontology with history and politics. Ontology, or the history of the dispensation of being, issues in a kind of "politics of being." In the process, politics and history as more or less independent realms perpetrated by more or less free and responsible human agents entirely disappears, as does the traditional concept of "political judgment." Certainly, Heidegger himself, with his totalizing, politically indiscriminate visions of being, showed no political judgment.

A few stages in Wolin's interpretation merit special attention. He convincingly shows that the "pessimistic" philosophical anthropology of *Being and Time* and the ubiquity of existential fear and death easily merged with the deeply conservative revolutionary worldview of the German *mandarin intelligentsia* (Fritz Ringer's term) of the 1920s. Heidegger's predilection for Oswald Spengler's popular theme of the decline of the West, as well as his extensive preoccupation with the work of Ernst Jünger prepared him for his interpretation of Hitler and National Socialism. The latter, as Luc Ferry and Alain Renaut emphasized, was to provide Heidegger with the antimodern response to the challenge of global technology allegedly not provided by modern democracy. Heidegger's self-admitted "big blunder" was to join a movement that did not live up to the momentous historical task he had assigned it. Thus, Heidegger never relinquished his conviction of the "inner truth and greatness" of National Socialism even in the face of Auschwitz, the holocaust, and the other evils it perpetrated.

Another point Wolin makes is of even greater significance. It had been argued by both Habermas and Leo Strauss that Heidegger's practical failure to become the "führer of the Führer" had not only led him to his preoccupation with Nietzsche and the notion of Western history as the unfolding of nihilism but, finally, to history as the fateful dispensation of being itself. Wolin agrees that this step led to the total elimination of human agency and politics and to the fateful equation of truth with the disclosure of sheer temporality. But in addition, Wolin shows that this acceptance of "existential criteria" for political truth is profoundly exacerbated by Heidegger's "equiprimordiality of truth and error" (p. 118). Ultimately, that is, Wolin traces Heidegger's incapacity for political judgment to his incapacity philosophically to distinguish truth from untruth. Briefly, Heidegger's notion of truth lying in the unconcealment of being contains a necessary concealment in the act of unconcealment. Thus, the errancy resulting from the partial concealment of being is part of the primordial essence of truth as unconcealment; and thus, truth disables us ever to escape errancy or to distinguish the two. Consequently, Heidegger always rejected the notion of an ethics. His notion of truth and its necessary errancy also made nonsense of his attempt personally to mythologize himself as the fated voice of being.

Lastly, Wolin's otherwise admirable treatment of the subject suffers from one internal inconsistency. He begins his study with a sense of caution: he only wants to focus on Heidegger's political philosophy, not his philosophy per se. He is not persuaded that the results of his study will transfer to Heidegger's essential philosophy except in a "highly qualified and tentative fashion" (p. 15). But as Wolin gets going, it becomes increasingly clear that he is challenging the most quintessential elements of Heidegger's thought—like his fundamental notion of truth as unconcealment. What, if not his concept of truth, is Heidegger's philosophy per se? Wolin ends his book by speaking of the "bankruptcy" of Heidegger's later thought. He even doubts "the underlying cogency of his conceptual framework in its entirety" (pp. 168–69). And he agrees that Heidegger "betrayed" philosophy. Wolin's proving considerably more than he planned unfortunately leaves the relation between a "first philosophy" and practical politics in a state of confusion. Nevertheless, I have been persuaded by Wolin to believe Karl Jasper's early dictum that Heidegger's way of thinking is "essentially unfree, dictatorial and uncommunicative."

University of Colorado, Boulder HORST MEWES

Justice and the Politics of Difference. By Iris Marion Young. Princeton: Princeton University Press, 1990. 286p. $45.00 cloth, $10.95, paper.

The universe John Rawls created has once again expanded. This study by Iris Young brings together a series of essays that have as their common focal point the attempt to transcend the distributive paradigm of justice most famously associated with Rawls's theory of justice. Starting with a critique of that paradigm, Young advances a theory of oppression—which she then expands upon in chapters on welfare capitalism, the biases inherent in the idea and practice of neutrality, the discipline of bodies, difference feminism, affirmative action, city life, and (finally) international justice.

Young's study has the great merit of being simultaneously lucid and politically passionate. Attempting to confront the theory of justice with the harsh realities of the ongoing injustice wrought by the systemic inequalities of the late capitalist world order, she is relentless in her pursuit of the ideal of justice. She is also conversant with the most recent developments in contemporary critical and poststructuralist theories, making good use, for instance, of insights from Foucault to refute the notion of power as a distributed good and Kristeva's idea of abjection to advance an explanation for racism, sexism, and homophobia.

Young moves between the worlds of poststructuralism and critical theory smoothly, and she is consistent in her effort to make political theory useful for the expansion of just possibilities. The book's merits are many; yet, in the end, I was troubled by it. Although it qualifies even its strongest conclusions regarding the need to remedy unjust situations, at its core, this book—by virtue of its very power as a statement concerning the extent of injustice—gives expression to the limits of the idea of justice as a political end in the contemporary world.

I do not wish to condemn or dismiss what seems to me to be an excellent work of scholarship and care, and I know that many of the few questions I can raise go beyond the scope of the book; yet it seems to me that in *Justice and the Politics of Difference,* Young has inadvertently closed, rather than opened, debate concerning justice and difference by defining injustice in such a monolithic way.

Her pivotal chapter is entitled "The Five Faces of Oppression." Arguing that it is necessary to develop a theory focusing on how the contingencies of group identity may engender oppressions, Young makes a key leap. Taking issue with the notion that to treat people as members of groups, not as individuals, is the best way to advance justice, she suggests that "it is foolish to deny the reality of groups." (p. 47). She then, however, assumes that through state policies to address pernicious group distinctions, progress toward justice will be made. As malign as the neglect of rights has been in the United States and other capitalist societies, there is no reason to assume—and much reason to deny—that such an alternative approach would help lessen the group oppressions she identifies. Moreover, once Young gets down to specifics, minor and major injuries are practically indistinguishable. The accumulated effect of her analysis of the extent of injustice will be for her less-sympathetic readers, either despair or, more likely, dismissal.

For Young, the reach of injustice is so thorough and so multifarious that there is scarcely a person who does not suffer it. Of course, this may well be true. I have no difficulty in accepting the ubiquity of injustice in the world, but wonder which injustices may be remedied through the use of policy and which not. Young fails to ask this. More troubling, in this book rarely is a positive assertion regarding a goal of justice (e.g., the need for group participatory democracy) assessed in reference to the injustices and miseries that would accompany it. Indeed, despite her own cautions, Young embraces all sorts of intrusions by state power into the lives of people in order to ameliorate conditions that might better be left alone by such a form of power. Having embraced Foucault's discussion of power in her critique of liberals, she abandons it when it comes to her quasi-socialist program.

If the Rawlsian juggernaut is finally reaching its terminus, it may well be that *Justice and the Politics of Difference*—as resistant as it is toward Rawls and as powerfully as it asserts a need for difference within a community—may be one of the more interesting variations his work has inspired. Yet it may also provide reason for rethinking the politics of justice from other, perhaps more transgressive, perspectives. In the meantime, it will be interesting to see what Young does next. A mind so engaged will not rest where this work concludes.

Amherst College THOMAS L. DUMM

AMERICAN POLITICS

The Exemplary Presidency: Franklin D. Roosevelt and the American Political Tradition. By Philip Abbott. Amherst: University of Massachusetts Press, 1990. 233p. $35.00 cloth, $13.95 paper.

Looking Back on the Reagan Presidency. Edited by Larry Berman. Baltimore: Johns Hopkins University Press, 1990. 332p. $45.00 cloth, $13.95 paper.

The Bush Presidency: First Appraisals. Edited by Colin Campbell, S.J. and Bert A. Rockman. Chatham: Chatham House, 308p. $25.00 cloth, $16.95 paper.

The Nixon Presidency: Power and Politics in Turbulent Times. By Michael A. Genovese. Westport: Greenwood, 1990. 265p. $42.95.

These politically wide-ranging books cover most of the modern to the postmodern presidencies. Although the topics and approaches are diverse and numerous, the books have one thing in common: the quality of the scholarship is extraordinary. Abbott's book, *The Exemplary Presidency*, is by far the most profound and innovative not only of those reviewed but of the more recent scholarly literature. It provides a theoretical framework that elevates presidents to a higher level of historical interpretation and political thought than previously assumed by scholars.

For years, participants, students, and presidential scholars have consistently shown that Americans value brilliance of thought but that a deep and comprehensive philosophical outlook has escaped them. Abbott challenges this conceptual framework and, instead, asserts that the "American presidency as an institution . . . is in a fundamental sense exemplary" (p. x). To Abbott, presidential statements are models for political thought. Presidents are put in a position to articulate political philosophy, and they "do speak in the language of political philosophy" (p. 7). For these reasons, the presidency should be studied as a structure for the creation of political philosophy. From this vantage, Abbott attempts to demonstrate "how complex this process of imitation can be and . . . how much eloquence and profundity it requires" (p. x).

This philosophical approach has significance not only because it is rarely used but because, traditionally, presidents have centered their presidencies on the actions and thoughts of former presidents. As Abbott reaffirms, contemporary presidents carry with them "the successes and failures of past presidents as burdens to be born, or as opportunities to be explored" (p. 14). Furthermore, Abbott provides convincing evidence of how FDR constructed his political and philosophical outlook from those of Hamilton, Jefferson, Jackson, and Lincoln. These constructs (more vividly exemplified by Lincoln's) asserted that there was a core of beliefs that defined us as Americans and that through imagination and understanding of history, presidents have rallied their thoughts and policies around these core beliefs.

Despite the author's consistency in using these philosophical contructs, it seems he at times stretches Roosevelt's imagination to fit his conceptual framework. It is also difficult to believe that as a political pragmatist, FDR was sufficiently well-grounded in philosophical matter to appreciate and apply Hamiltonian and Jeffersonian political philosophy. These shortcomings do not prevent the author from providing scholars and students of the presidency with a research framework for years to come.

The political nature and modus operandi of Richard Milhous Nixon have inspired the production of a wealth of information on this controversial figure. One wonders why Michael A. Genovese has written yet another account of the power and politics of the Nixon presidency. Yet the author claims (with justification) that it is the least understood of modern administrations. In other words, there is no "comprehensive overview of the Nixon presidency" (p. 1). Accordingly, Genovese tries to piece together the many puzzles that characterize Nixon and his presidency. To unravel this mystery the author begins this well-researched study with a brief biographical sketch of Nixon's early years and his rise to political office followed by a study of Nixon's personality; style of decision making and management; and handling of domestic, economic, and foreign policy. He concludes with an examination of the dissolution of Nixon's presidency. Genovese concludes his study with "an analysis of the impact and legacy of Richard Nixon on the presidency and the United States and tries to place Nixon into a broader comparative perspective with other presidents" (p. xii).

Considering the hidden and obscure nature of Nixon's presidency, Genovese presents his strengths and weaknesses in an objective manner. Although I agree with the author's conclusion that "fundamentally, Nixon has no central philosphical beliefs," (p. 238), there are some contradictory characteristics of the Nixon presidency that fit Abbott's conceptual framework outlined in *The Exemplary Presidency*. Because of Nixon's psychological motivations (which included childhood poverty and provincial status), he relied on FDR-style tactical skills. More important, Nixon's political disposition is both Rooseveltian and Jacksonian. His distrust of the Eastern establishment, his hatred of power derived from privilege, and the concept of the "silent majority" are all fundamental elements of the Jacksonian political philosophy. In spite of the author's heroic efforts to provide an impartial analysis of Nixon's domestic, economic, and foreign policy, the tone of the book is strongly affected by Nixon's unique personality—ultimately, his "fatal flaw."

The last two books are edited volumes covering the two most recent presidents. *Looking Back on the Reagan Presidency* consists of a series of conference papers written by a group of distinguished scholars. These Brookings Institution conferees review the impact of the Reagan administration in five general areas: foreign policy, economic and fiscal policy, institutional changes, elections, and congressional relations. Although the authors have many divergent points of view, it seems clear that they are in agreement that the administration's impact includes "a detachment from policy; an ideologically derived commitment to specific policy priorities; and the captivating, disarming personality of the man himself" (p. ix).

The impact of the Reagan administration on both domestic and foreign policy seems stronger than anticipated. Moreover, his ability to continue to govern was based on what Thomas Mann calls "a protective rather than an expansive domestic agenda after the first six months of his administration" (quoted p. 26). Yet there were failures. Considering the fiscal rhetoric of this conservative administration, the increase in size of the federal deficit and its impact on future generations proved to be quite embarrassing. Furthermore, his inability "to achieve a realignment with the Republicans as the new majority party in this country" (p. 28) was another unfulfilled goal. In foreign policy, the reaction is mixed. In "The Middle East," Robert Lieber argues, "there has been somewhat greater latitude for the use of military power and for unilateral action than appears to be the case in the late 1970's" (p. 51), while Robert Pastor concludes that against Reagan's policies in Latin America "the judgment will be harsh, and not just by critics" (p. 47).

A very important feature of this volume is the significant use of quantitative data to explain the impact of the Reagan legacy. Walter F. Murphy's "Reagan Judicial Strategy" and Gary C. Jacobson's "Meager Patrimony" illustrate the urgent need to use quantitative data in the study of presidential politics.

The Bush Presidency, edited by Colin Campbell and Bert A. Rockman literally provides the first serious assessment of the Bush administration. Like the Reagan study, *The Bush Presidency* is a comprehensive assessment of the administration's performance. Both deal with similar topics—foreign and domestics politics—and both involve a group of distinguished scholars. Nevertheless, *The Bush Presidency* uses a different approach and arrives at rather different conclusions. The authors "describe events that have been relevant to various aspects of the Bush Presidency" (p. xii), ranging from Bush's leadership style and conduct of foreign policy to his relationship with Congress, the voters, and the bureaucracy. The central theme, however, is based on Bush's leadership style and effectiveness given the complexity of our system of government, the weak party system, and a divided government. It is an evaluation of performance as it relates to personality and style. As with Nixon and Reagan, the evaluation of Bush's performance is a mixed bag.

It is still too early to specify Bush's presidential exemplars definitively. But like his immediate predecessor, Bush focuses on FDR as an exemplary president. Thus, the Bush presidency, too, is analyzable within Abbott's framework, further underscoring the pervasive influence of FDR, as Abbott argues when he states that "to the extent to which the exemplary presidency of Franklin Roosevelt represents the exemplary presidency in general as a mode of governance, future presidents will indeed always remain the children of FDR however much they seem to reject their father" (p. 201).

University of Texas, El Paso ROBERTO E. VILLARREAL

Ethnic Identity: The Transformation of White America. By Richard Alba. New Haven: Yale University Press, 1990. 374p. $35.00.

The American Kaleidoscope: Race, Ethnicity, and the Civic Culture. By Lawrence H. Fuchs. Hanover: University Press of New England, 1990. 618p. $40.00.

Social science inquiry on race and ethnicity has spanned a continuum ranging from broad theoretical treatments to rich case studies of particular ethnic communities. Decades of such investigation notwithstanding, researchers and policymakers continue to ask fundamental questions about ethnicity in the United States. Furthermore, these questions are framed at all levels of analysis from nation–state to individual. What sustains a national identity—a sense of membership—in the wake of growing ethnic diversity? What determines the degree to which different ethnic groups align themselves with a national identity? What does it mean to an individual to be a member of a particular ethnic group? How does this meaning change over time and across space?

Two new works grapple with these questions, one casting its net wide across the entirety of U.S. history, the other focusing deeply on the ethnic experiences of a single community at a single point in time. Together, Lawrence Fuchs's *American Kaleidoscope* and Richard Alba's *Ethnic Identity* advance ethnicity research by providing cogent analyses of ethnicity as a profound influence on U.S. public policy and as an ever-changing element in the construction of personal identities among individuals.

Two ambitious themes frame *The American Kaleidoscope.* Part 1 assesses ethnicity as a major factor in the development of U.S. national identity. Fuchs reviews how the state has managed ethnic diversity over time and what conflicts have arisen from these strategies; part 2 places ethnicity in its current policy context in areas as varied as education, employment, civil rights, residential settlement, and political participation.

For Fuchs, the basic demographic process engendering struggle in the creation of U.S. national identity was from the start, is now, and ever shall be international immigration (a topic Fuchs is well equipped to address as past director of the U.S. Select Commission on Immigration and Refugee Policy in the late 1970s). Fuchs traces the influence of immigration and immigrants upon elite and folk visions of Americanness from colonial settlement to the present day. In doing so, he draws a useful distinction between two kinds of "ethnic pluralism" evident in the historical record: a voluntary pluralism, characteristic of European ethnics, and a "coercive pluralism" (or enforced separatism) characteristic of the African, Mexican, Asian, and Native American experiences. The policy mechanisms of exclusion differ for each of these nonwhite ethnic groups, from slavery to enforced labor migration and repatriation—mechanisms that Fuchs outlines in admirable detail. He then argues that the powerful logic of the U.S. civic culture, with its promotion of participatory democracy and equal opportunity, gradually undercut support for separatism and exclusion of nonwhites, replacing such approaches with a popular conceptualization of ethnicity closer to the voluntary pluralism ideal—in which ethnic diversity exists and has meaning for people but no longer means necessarily constrained life chances for nonwhites.

In keeping with his portrayal of a U.S. civic culture where laws and civil rights forge the ties that bind (as opposed to ethnically based claims of membership), Fuchs devotes considerable attention to the intersection of policy and ethnicity. He interprets the policy record on ethnicity as the demolition of state-sanctioned separatism protecting white interests and the simultaneous emergence of state-sanctioned pluralism promoting the interests of ethnic minorities through such initiatives as affirmative action and bilingual education. Fuchs concludes that many of the xenophobic fears behind restrictive immigration policies and English-only movements are doomed to political failure, since they violate the civic precepts of fairness and freedom. He ultimately takes a page from William Julius Wilson's *Truly Disadvantaged* (1987) in his assessment of current policy trends in ethnicity; that is, Fuchs charges that race-specific attempts to redress historical wrongs have done little to improve the lot of most nonwhites, particularly those of the urban underclass. His prescriptions call for a move beyond race-specific policies to race-neutral, class-based antipoverty initiatives aimed at all those in need.

The theme of a nation whose civic culture accommodates, and eventually transforms, the meaning of ethnic diversity finds corroboration in *Ethnic Identity*, Richard Alba's exhaustive survey analysis of one white ethnic community in upstate New York. With carefully constructed hypotheses derived from both psychological and sociological literatures, Alba seeks the meaning of ethnicity in the daily lives of individuals whom Fuchs might call the inheritors of the voluntary pluralism tradition. Alba's analyses dissect aspects of ethnic experience as varied as ethnic food consumption, ancestral language use, ethnic endogamy, and settlement in ethnic neighborhoods. He concludes that the traditional anchors of ethnicity for white Americans—undivided ethnic ancestry and attachment to ethnic social structures like churches, neighborhoods, and community groups—are giving way to more individualized, symbolic expressions of ethnic identity. In other words, the Alba survey respondents *feel* ethnic (reporting their ethnicity as an important element of their identity) even when they do not *act* ethnic in the sense social science has historically understood, that is, by marrying ethnic fellows, speaking ancestral languages, or participating in ethnic social organizations. From this research, Alba envisions the emergence of a new "European American" ethnic category, identifying, in a general sense, with an immigrant experience in the first 150 years of American nationhood and restricting its expressions of this identity to symbolic practices that do not much influence daily life.

In this sense, Alba acknowledges and corroborates Herbert Gans's *Middle American Individualism* (1979). However (Alba insists), at least two caveats should be attached to Gans's view that such "symbolic ethnicity" implies a trajectory toward the deterioration of ethnicity as a meaningful concept. First, Alba notes that ethnic identification appears as significant among younger cohorts as those closer to the immigrant experience and that it is more vigorous among more highly educated respondents (although he admits that his cross-sectional data do not allow him to trace such identification patterns over time). Second, Alba speculates that ethnic identification among whites may persist, at least in part, as a function of the demographic momentum of other ethnic populations (e.g., Latinos and Asians), who promote and perpetuate their own ethnic identities. Regrettably, Alba does not explore the possible political, economic, or social implications of such reactions on the part of white ethnics—a thesis worthy of continued study.

While the theme of ethnicity undergoing transformation emerges from both of these works, the volumes could hardly be more different in tone, style, or methodology. Fuchs draws upon copious historical, archival, and secondary data sources spanning three centuries, while Alba methodically probes the responses of 524 survey respondents in a single community in an effort to uncover precise patterns of ethnic ancestry and associated trends in ethnic identity. Not surprisingly, the differences in approach lead to different points of critique.

Fuchs admirably weaves together a massive amount of information. However, no tables are presented, leading to some confusing narrative summaries of empirical evidence. More importantly, Fuchs's effort to cover so much ground results in his drawing upon evidence that is, at times, highly selective. This is particularly evident in his treatment of the politics of language, where he relies disproportionately on newspaper accounts of ethnic conflict over bilingual education in the southwestern United States and on the autobiographical essays of Richard Rodriguez, whose dramatic social commentary is often invoked as a rationale for policy reforms few U.S. Latinos actually support, like the dismantling of bilingual programs. Such evidence is emphasized at the expense of many widely available scholarly efforts to evaluate bilingual education more systematically, such as Cummins's *Enpowering Minority Students* (1989). In contrast, Fuchs's treatment of immigration policy is most compelling. He sounds a much-needed, well-documented note of reassurance to those who fear an onslaught of foreigners, deftly illustrating the contribution of international migration to the social and economic fabric of the United States.

Alba's micro approach cuts a narrower path through questions of ethnic identity, adhering closely to the classic social science treatment of a research question. Theories are reviewed, hypotheses formulated, and data analyzed and discussed in meticulous detail. Alba, however, makes little attempt to place his findings in any sort of political or public policy context. Several provocative questions emerge from his analyses. For instance, does the transformation of ethnicity among whites into an amorphous, symbolic identity imply reduced significance of political mobilization around issues of ethnicity for these groups, or might such mobilization simply take an equally amorphous "whites-verses-nonwhites" tone? Does the changing nature of white ethnic identity undercut white support for race- or ethnicity-specific public policies or for immigration policy reform? In what ethnic terms do whites and nonwhites in the Alba community view each other, and what are the implications of these views for political conflict and/or coalition building? With his unwavering attention to his white-ethnic-European-American sample, Alba thoroughly highlights one pattern in Fuchs's American ethnic kaleidoscope. The next task will be to understand how this pattern influences the potential social and political patterns of other groups in times of rapid ethnic compositional change.

University of Arizona SUSAN GONZALES BAKER

Purchasing Power in Health: Business, the State, and Health Care Politics. By Linda A. Bergthold. New Brunswick: Rutgers University Press, 1990. 213p. $37.00.

According to most published accounts, corporate America has not played a proactive or concerted role in health care reform. Corporate interest has been traced to the inflation in hospital expenditures and the economic recession of the early 1980s. Corporations responded by implementing cost containment measures (e.g., cost sharing, HMOs, PPOs, and wellness programs) for their own work forces, not by shaping national health policy.

Linda Bergthold challenges this account in her book, *Purchasing Power in Health.* She sets out to demonstrate that business not only involved itself in health reform as early as 1969 but also developed as an organized interest group in health care politics.

Bergthold's agenda is ambitious. She seeks to combine case studies with survey evidence in order to illustrate business activity at federal, state, and local levels with a view to documenting widespread business participation in health care policy formation and implementation since 1969. She suggests that business participation in health policy formation is evident in three types of interventions: institutional mechanisms for change, such as coalitions and lobbying groups; advisory bodies, such as policy committees; and direct legislative participation by business leaders. At the federal level, she describes the genesis and activities of the U.S. Chamber of Commerce, the Business Roundtable, the Washington Business Group on Health, and the Dunlop Group. At the state level, she analyzes the role of business in California's MediCal legislation and Massachusetts' hospital prospective payment system, both enacted in 1982. She also compiles survey evidence from three studies of state business coalition activity and health policy involvement to support the conclusions from these case analyses. Finally, at the local level, she briefly discusses the prevalence, origin, and process of business–health coalitions.

This broad agenda is perhaps too ambitious. The author has understandable difficulty interweaving the case studies with survey data collected by others, as she also does interrelating the activities of business leaders at the different geographic levels of analysis. As a result, the wide array of business leaders, groups, and events described here do not cohere.

Equally troubling are several weaknesses in Bergthold's argument. One is her failure to justify 1969 as the starting point of business involvement in health policy. According to her argument, 1969 witnessed a blending of economic and political motivations for business participation, "the beginning of a decline in corporate profits accompanied by a national involvement of both business and government in reorganizing and rationalizing health care" (p. 22). While the economic motivation makes sense, Bergthold never confronts evidence that such motivations were not keenly felt until much later, in the early 1980s. For example, she never explains why business dragged its feet during the 1970s on such health reforms as offering HMOs to employees or why a 1980 survey concluded that "corporations were neither greatly concerned nor strongly motivated to do much about their health care costs" (Sapolsky et al., "Corporate Attitudes toward Health Care Costs," *Milbank Memorial Fund Quarterly* 59). Conversely, one can argue that economic motivations actually developed earlier in some cities due to a "strategic orientation" among business elites to control health care cost escalation for their communities. Such motivations surfaced in Minneapolis during the late 1960s and early 1970s with the formation of the Twin Cities Health Care Development Project. This coalition inexplicably receives no mention in the book. The political motivation for business participation, on the other hand, is never explained.

A second, related problem is the book's thesis that business supported or created institutional mechanisms during the 1970s to implement their health policy agenda. There is insufficient evidence here of a coherent business agenda for health policy in the making during this period. Some of the institutional mechanisms Bergthold describes did not exist during the seventies; others were neither created, nor dominated, by business (as the author admits). Oftentimes, Bergthold's account of business's role in policy formation is too vague: "Business exercised its power instead in more subtle ways, stemming from the structural power of business in society" (p. 83).

As a result of these problems, the reader cannot reject the alternative hypothesis that business is a late entrant to health policy development. Nor is the reader convinced that until recently, business has been anything more than a follower of government initiatives to control health care spending. Most of the case study and survey evidence dates from the 1980s. Moreover, the case findings from California strongly suggest that business was used by state democrats as a lever to sway Republicans and counteract providers.

Despite these problems, Bergthold's synthesis is informative in several respects. The book traces both the historical development of several major business coalitions (primarily at the federal level) and the roles played by key corporate and political leaders, as well as rich case studies of their involvement in specific pieces of health care legislation. The book also insightfully identifies some of the inherent conflicts that can undermine business coalition activity, such as business's multiple roles as "purchaser," "insurer," and "hospital trustee." Finally, the book points to the growing interaction, or "fusion," of public and private power in the formation of health care policy. This fusion of interests constitutes the major coalition in Bergthold's book. The business–government alliance is now successfully confronting the hospital–medical industry over such policy issues as payment and quality review.

Business power in health care purchasing and policy is clearly on the rise. Bergthold provides some excellent historical background on key business coalitions and associations that will likely play a major role in future reforms and sketches some profound issues emerging from business involvement. The lack of integration and convincing evidence regarding the timing of business involvement should not obscure these important contributions.

University of Arizona LAWTON R. BURNS

Long-Wave Rhythms in Economic Development and Political Behavior. By Brian J. L. Berry. Baltimore: Johns Hopkins University Press, 1991. 241p. $49.50 cloth, $19.95 paper.

In the ranks of analysts who find long-wave arguments attractive, rarely a move is made without some trepidation. There are certainly more popular, more paradigmatically sanctioned, and therefore more prudent courses of action within academia. Sometimes, however, the data literally grab you by the throat and seem to leave few choices other than to take cyclical explanations seriously. Brian Berry describes such an experience as his initiation into the much-contested arena of long-wave rhythms. Examining the relationship between economic growth and urban migration patterns in the United States since 1790, Berry found that accelerations and decelerations in economic growth, once smoothed by decadal averaging, took the form of half-century fluctuations accompanied by similarly patterned bursts and declines in the pace of urban migration. Once hooked, Berry felt compelled to explain this relationship and was thereby seduced into exploring the long-wave literature.

But Berry's book is not about processes of urban migration. There are two other facets of the long-wave conversion process. One is that whatever topical interest with which you begin, an interest in long-wave explanations forces an analyst to come to grips with the root causes of long waves. The phenomenon is not unlike an explorer of a river's tributary branch becoming obsessed with finding the river's source. A second by-product of delving into the long-wave literature is an initial reaction of dismay at its intellectual disorder. A compulsion to set things right and bring order to a confused and confusing set of arguments and evidence is one possible outcome. As Berry describes the subfield "It is a difficult land, populated by tribes with different cultures and languages, separated by their world-views and by the boundaries they defend" (p. xiii). What he claims that he wants to do is to provide readers with a better map of this difficult land than has previously been available.

Berry's reference to the difficult terrain of long-wave analyses is more accurate than is his claim to have executed a superior mapping of its difficulties. Rather than explore all the hills and valleys (one viable way to map the landscape), Berry has decided to take a major shortcut or two. Instead of surveying all the explanatory paths, Berry chooses to focus on the route that he finds most efficient for explaining why economic and other types of long waves exist.

He also chooses to do it in what comes across as a highly inductive exercise. One of his most serious complaints about the literature he is addressing is the poor quality and paucity of appropriate data analysis. Thus, we are bombarded with 137 figures to take us through a series of exercises demonstrating behavioral regularities, often with the aid of an unusually user-friendly form of chaos mapping.

The type of behavioral regularity that Berry finds most useful to explain long waves is a roughly 25-30 year Kuznets cycle in infrastructural investment. Two of these Kuznets cycles are embedded within each Kondratieff long wave. The typical movement can be seen as beginning in a long-wave depression, moving to a period of inflationary growth (in the first Kuznets), and followed by a phase of stagflation, a period of deflationary growth (in the second Kuznets), and the next long-wave depression. These alternations in growth and prices set up longer-term fluctuations in technological change. Established technologies saturate their markets in the first and inflationary Kuznets. New technologies emerge in the second and deflationary Kunzets.

Berry advances an interesting argument that may hold up to further scrutiny. His argument, which centers on a specific causal dynamic frequently absent in long-wave analyses, is certainly testable and also has novel qualities, especially in terms of the alternating Kuznets sequence. But much more work remains to be done. What Berry demonstrates is that there is supportive evidence in United States economic data for the sequencing that he describes. Which processes drive what other processes still needs to be worked out theoretically and empirically. Moreover, it also remains to be seen just how uniquely American his economic sequencing is. If other economies are subject to long waves without the dependence on paired Kuznets, what might that suggest for Berry's preferred explanation?

Berry might have pursued some of this further modeling. Instead, he devotes chapters to speculations about such related topics as war, critical elections, mass psychology, demographic implications, stock market indexes, and climate. The chapter on critical elections and realignment processes is useful in the sense that the relevant political science literature usually stops short of linking the political outcomes explicitly to the underlying, long-term, economic fluctuations. Yet these chapters fall well short of doing justice to the complexities of their subject matters. In some cases, as exemplified by the chapter on war, Berry makes some of the same types of weakly grounded, loose statements that he criticizes other long-wave analysts for. Hence, these auxiliary chapters are at best suggestive.

This book does not provide a comprehensive analysis of long-wave rhythms in political economy. It does advance a provocative argument for why there might be long-wave rhythms. It generates persuasive empirical support for the strong argument that is advanced. Both are welcome contributions to a growing, increasingly sophisticated, and genuinely international research program.

Indiana University William R. Thompson

Governors, Legislatures, and Budgets: Diversity across the American States. Edited by Edward J. Clynch and Thomas P. Lauth. Westport: Greenwood, 1991. 191p. $42.95.

This is a study of how the budgetary process works in practice, at both the executive and legislative levels, in 13 American states, with the emphasis on events during the 1980s. The chapters are written by experts in each of the states, an approach that is often necessary in comparative state studies and that works well in this volume. The editors have provided reasonable guidelines; and these are usually followed by the authors, so that similar topics are covered in each chapter.

Each of the brief state chapters is tightly packed with detailed information. To make this a valuable comparative study, there needs to be a concluding chapter, synthesizing the findings and explaining what practices and trends are common to the states, the differences among them, and why these exist. The editors' final chapter is designed to answer such questions; but they do so too briefly, in only seven pages, emphasizing primarily legislative–executive relations.

Several common patterns and trends stand out. The process of budgetary review has grown more sophisticated in all the states, partly because of the use of computers. In most states, the governor has gained more direct control over the executive budget. Most state legislatures have developed a more competent and better-staffed committee structure for budgetary review.

There are differences among the states in whether the balance of power in budget making is shifting to the executive or the legislature. Clynch and Lauth conclude (correctly, I believe) that there is a trend toward greater balance, a shift away from domination of the budgetary process by either branch. There are a few states, however, such as Texas and Mississippi, where staffs of both the governor and legislative committees prepare budget estimates, and the legislature pays more attention to its own committee recommendations.

It is also clear from this book that no matter how well organized and staffed the budgetary process seems to be, a serious financial crunch will play havoc with the system, often leading to special sessions and deadlocks, particularly when there is a partisan division of government. The chapters include several examples of this, particularly during the early 1980s. Texas and Kentucky illustrate well the impact of severe economic setbacks on budgets. Even more dramatic examples have occurred in the 1991 legislative sessions, since this book was written.

Unfortunately, this book tries to cover too much too briefly. The state chapters, which average only 11 pages each, describe the formal executive and legislative budget-making process in some detail. But often, the political realities are skimmed over too briefly. For example, the California chapter barely mentions the constitutional requirement that the budget receive a two-thirds vote in each chamber, a reality that has enormous political implications. The Illinois chapter tells us only briefly about the governor's aggressive use of the amendatory veto and its impact on the whole budgetary process.

Some of the chapters provide interesting detail about the political aspects of budget making. In Connecticut a strong partisanship pervades the budgetary process. In Texas, the election of a Republican governor changed the political dynamics of the process, while in South Carolina a governor aggressively challenged the traditional legislative control of budget making. Florida illustrates the importance that conference committees can play.

The editors, in their concluding analysis, might have given more attention to the political dynamics of budget making, such as the importance of divided government and the problems raised when economic problems require severe budget cutting. It is important to understand how the political battles over state budgets have been affected by the shifting of federal responsibilities to the states; the public clamor for more services and fewer taxes; and the continued inability of state governments to control the state's economy (at least in the short run), which makes rational budgetary planning impossible.

University of Kentucky MALCOLM E. JEWELL

Contested Ground: Collective Action and the Urban Neighborhood. By John Emmeus Davis. Ithaca: Cornell University Press, 1991. 356p. $45.00 cloth, $16.00 paper.

Over the past 10 or so years, urban scholars have undertaken a reconstruction of urban theory based on the political and economic restructuring of the last several decades. This project has been interdisciplinary in its scope, and the resulting insights have effectively refined and expanded our understanding of the processes behind urban growth and change. Yet while new insights and understandings have been generated at the level of political and economic structures, insufficient attention has been paid to theorization below the urban level at the smaller scale of the neighborhood. In *Contested Ground,* John Emmeus Davis, an urban planner by training, makes a significant theoretical contribution to illuminating this poorly understood yet critically important area by moving our understanding beyond well-worn broad generalizations about the class-based nature of community action. The central aim of this book is to begin the process of developing a theory of domestic property interest group formation based on an empirical exploration of the conditions under which territorially based collective action occurs. Toward that end, Davis advances a theoretical framework that combines a static model of property interest groups with a dynamic model of group formation and intergroup conflict. In short, Davis examines objectively observable and subjectively held interests and conditions in order to explain why and when consciousness and organization combine to produce a collective action that is fundamentally tied to place and property.

Contested Ground is divided into three parts with a total of 13 chapters and two appendixes. In part 1, Davis provides a comprehensive and critical treatment of the Marxist and Weberian perspectives on locality-based collective action. Davis's objection to the Marxist perspective is that it tends to misrepresent all struggle as class struggle. He argues that such an approach is theoretically impoverished and insists that urban collective action is most often based on complex territorial, not class-based, interests. Davis is also dissatisfied with the Weberian attempts at theorizing housing classes and contends that an emphasis on equity as the only interest that is causally significant severely limits the explanatory power of such an approach. Part 1 is completed with a careful and detailed elaboration of an alternative theory of territorially based collective action derived from an ideal typology of material interests and a three-stage model of housing classes based on *propertied*-versus-*propertyless* categories. Davis argues for 10 distinct property interest groups distinguished along the axes of tenure, function, and interest. His stage model of group formation is based on the interaction of levels of "housing consciousness" and "housing organization" that translate into three, potentially progressive, phases of intergroup conflict.

In part 2, Davis tests his theoretical framework empirically by way of a longitudinal case study of collective action in Cincinnati's West End community. The West End, like many inner-city areas of older industrial American cities, has gone through several waves of investment and disinvestment and was the object of large-scale urban renewal efforts in the 1960s and limited gentrification in the 1980s. Its population composition over the years has, for the most part, paralleled national migration and immigration trends. It is currently home to about thirteen thousand residents (many of them African–American), who hold varying tenancies. The case study of this diverse and currently divided neighborhood spans the years between 1800 and 1985 and is based on participant observation, documentary sources, and interviews with activists and representatives of locally important institutions. Davis expertly weaves theory and raw materials, providing a fascinating account of solidarity and fragmentation among West End neighbors, as well as a compelling argument for his interest-based framework. In part 3, Davis refines his theoretical framework based on the evidence from the case study and provides discussion of the usefulness of his approach for practicing urban planners and community organizers.

Contested Ground is an important advance in an evolving urban theory that is drawing upon, and contributing to, developments in social theory. Davis has done a great service for all of us by developing a theoretical approach to locality-based collective action that makes connections to theories of the production and reproduction of urban space. The case study of Cincinnati's West End invites comparisons and further theory testing—perhaps a focus on the impact of group interests such as gender or ethnicity as they crosscut domestic property relations. At both the theoretical and the empirical level, this is a competent, convincing, and stimulating piece of scholarship that should be on the bookshelf of every urban scholar.

University of Arizona SALLIE A. MARSTON

What Older Americans Think: Interest Groups and Aging Policy. By Christine L. Day. Princeton: Princeton University Press, 1990. 240p. $24.95.

Demographic Change and the American Future. By R. Scott Fosler, William Alonso, Jack A. Meyer, and Rosemary Kern. Pittsburgh: University of Pittsburgh Press, 1990. 280p. $29.95 cloth, $14.95 paper.

The needs of the elderly in the coming century are important today not only because we care about older persons but because we also care how we ourselves will age; how we will relate to our families; and how, and with what values, our children will grow up. High on the list of concerns is to determine how we will meet the increasing health care requirements of a rapidly aging society and pay for the benefits all of us expect from our society. A recent report projects that "demographics, living arrangements, disability and income will be the major determinants of the future needs of the elderly" ("The Needs of the Elderly in the Twenty-First Century").

We can anticipate large numbers of disabled elderly, an increase in the number of elderly persons who are institutionalized, and an increase in the numbers of elderly individuals living alone. And there will be significant numbers of subgroups of old persons who are at risk financially. Most importantly, we have enough information to make it possible for us to be constructively involved in planning for this future, to enable us to influence our future.

It is highly likely that the domestic public policy debate of the next few decades will be focused on the impacts of changing demography on our society. This debate will be considerably enriched by the introduction of these two books. Both are well written and provocative and deal harmoniously with the companion issues of changing demography and the opportunity for constructive societal changes through the use of special interest groups.

Both books adequately help us separate from reality such myths as that what is good for the elderly is necessarily opposed to what is good for the society at large and that all old people will rise to defend any policy that benefits the elderly regardless of its content, value, or cost.

Day's book illustrates in a convincing way that collectively, "old people are no more supportive of government benefits for the elderly than are younger adults." Older people are as varied as are their political beliefs, social status, income, sense of health and security, and residence. People join age-related interest groups for many reasons, sometimes totally unrelated to their own political beliefs. This may include joining an organization for add-on benefits of membership that may include reduced rates for purchases.

The critical task for us as a society, to which this book contributes significant insight, is to learn to understand older persons (or people of any age) on the basis of their individual characteristics and needs. Although needs may be grouped collectively for the purposes of generating public policy, they should not be used to generalize about characteristics of the elderly. If this book does no more than drive home this significant point, it will have made an important contribution to the literature. How interest groups represent a particular segment of the society, how they are constituted, and some cautions about interest group participation represent a significant part of this book.

The book ignores the significant advocacy role of special committees on aging of both the House and the Senate, as well as concern about aging policies on the part of city, county, and state legislatures. All of these comprise very effective advocates, especially at the federal level where it intersects with their funding priorities.

Fosler and his colleagues' presentation uses demographics to highlight societal changes but clearly cautions that demographics, although seemingly quantitative and objective, may be misleading and misrepresent a changing phenomenon because they are "artifacts, stick figures attempting to represent fluid multidimensional realities." Unfortunately, this multiple presentation by several authors has resulted in repetition of content that is burdensome.

With the expectation that there may be one million centenarians in the United States by the year 2050, the authors argue for the need to understand the political constituency of the aging segment of the population and to evolve public policies that respond appropriately without denying the needs of other groups. It is a challenge to project an older population that includes the destitute, who need the assistance of society; those who are near poverty and also need help; members of a middle class, who expect to maintain their economic status through social security and other redistributive policies; and retirees well able to maintain their high standard of living as a result of their accumulated financial resources. The most difficult task involved is to balance the diverse needs of older Americans against the unfulfilled needs of many other segments of our society and to minimize the tension created by competition for limited resources.

Some important ideas that are different but in harmony are projected by the authors of these two books. One argues for such an alliance of advocacy groups as the program called Generations United, which has as its goal to minimize intergeneration disputes. The other volume emphasizes the significance of worldwide demographic changes on economic and social events in the United States and therefore argues for addressing the interdependence of changes across the life span. How we deal with our elderly has impact not only on the person who receives—or is denied—service but, from a wider perspective, on the total society, because it affects people of all ages and backgrounds. What may be required is the replacement of special interest political advocacy groups with a national advocacy effort on behalf of all in need, irrespective of age.

University of Arizona THEODORE H. KOFF

Military Organizations, Complex Machines: Modernization in the U.S. Armed Services. By Chris C. Demchak. Ithaca: Cornell University Press, 1991. 202p. $31.50.

Cornell University Press has added another gem to its fine series on national security affairs. Chris Demchak's *Military Organizations, Complex Machines* is an excellent analysis of the interaction of technology and organization in the employment of modern weapons. The specific technology examined is the M-1 Abrams tank, and the hosting organization described is the U.S. Army; but the attempt is to provide broader lessons for militaries and organization theory.

Demchak is at her best in the conceptual chapters, where the problem of organizational complexity is effectively explored. Here, the influence of the emerging Berkeley school of organizational analysis is clearly visible and fully acknowledged. We learn that complex systems are vulnerable to a series of surprises because of their large number of internal relationships, each of which can be disrupted in unknown and undesirable ways. These nasty surprises ("rogue outcomes") plague the systems, forcing their managers into a continuing quest for coping strategies. Limited resources frustrate the managers' abilities to respond quickly and successfully to the inevitable rogue sets cascading through the systems. As downtime grows, the capacity of organizations to meet external challenges weakens dangerously.

The M-1 tank was designed to reduce the army's battlefield uncertainties. It was intended to be faster, tougher, and more lethal than the Soviet tanks it might someday encounter at the Fulda Gap. To do this, the M-1 was to utilize the latest technologies, including a gas turbine engine, laser-assisted targeting, and advanced gun–turret stabilization techniques that would allow it to fire accurately while maneuvering. The army's procurement agency, beset with its own political and organizational uncertainties, promised Congress that the M-1 would also be more reliable and cheaper to maintain than predecessor tanks, thus justifying its higher purchase costs. In order to achieve this, much of the repair was to be pull-and-replace after diagnosis by specially developed automatic test equipment. Of course, not all of these promises were kept. Although it has excellent combat capabilities, the M-1 has poor fuel economy, relatively short range, and a cranky disposition and is a nightmare to maintain.

The bulk of the book focuses on the army's attempt to deal with the nightmare. Demchak, a reserve officer, apparently had the needed access to the operators and the required technical knowledge to understand the maintenance adjustments initiated. In short, what was expected to be simple became very complex indeed as field units and an overly lean support network strove to cope with the operating uncertainties of the tank. Demchak offers some—but not enough—comparisons with the introduction of other complex weapon systems into the air force and navy to make convincing generalizations of this experience.

Nevertheless, Demchak is ready to draw lessons, particularly as they relate to wartime operations. Complex procedures instituted to cope with the surprises of complex technologies, she argues, will increase the operational uncertainties in the rear, as well as on the battlefield, raising the risk of catastrophic organizational failure. Instead, she believes, we should cautiously weigh the pros and cons of adding technical complexity to organizations with difficult, dangerous tasks like the military. Simplicity, if not the main guideline, should be at least a major option.

The Gulf War, which occurred as the book's final editing was being completed, is dismissed as exceptional. There, the M-1 was shown to advantage, as was nearly every other of our very complex weapon systems. But that war revealed how the American military actually copes with the risks of complexity. Two full complements of tanks for the units assigned were sent, one after the other. There were spares for spares. It is only junior officers and organizational theorists who worry about scarcity. Senior officers know that the organizational goal in that war and all others is to avoid American casualties and, like American politicians, believe the promise that costly, complex technologies offer for achieving that goal. Complexity, it turns out, is the only acceptable option.

Massachusetts Institute of Technology HARVEY M. SAPOLSKY

Civil Rights under Reagan. By Robert R. Detlefsen. San Francisco: Institute for Contemporary Studies, 1991. 237p. $24.95.

Liberals would argue that the title of this book is an oxymoron. Certainly, there have been laments that Reagan's reconstruction of the federal courts has led to the diminution of civil rights, successfully implementing a part of the "Reagan agenda." Robert Detlefsen joins the laments about the Reagan administration. Surprisingly, his complaints focus not on the effectiveness of the Reagan administration regarding civil rights, for he argues that the Reagan administration was unsuccessful in that effort. He focuses, rather, on Reagan's failure to dismantle affirmative action policies in employment and busing in education. He notes that Reagan failed despite the fact that both policies are strongly opposed by the American public. The puzzle he addresses, then, is how a highly popular president failed to abolish a set of highly unpopular programs.

The obvious answer to this puzzle should be that the American political system is notoriously resistant to change. Factors like the influence of interest groups, the importance of bureaucracy in implementing policy, and the American tendency to compromise lead us to expect that radical change is difficult to achieve. Detlefsen recognizes this but argues that the real answer lies elsewhere: "[A] *civil rights elite* in America . . . has developed an ideology of civil rights that necessarily demands the creation of a jurisprudence in which different kinds or levels of rights and privileges are allocated to individuals on the basis of their membership in particular social groups, defined according to racial, ethnic, and gender criteria" (p. 13; emphasis mine). This civil rights elite has prevented the abolition of unpopular civil rights policies. Through an analysis of the opinions in several important legal cases dealing with affirmative action and school desegregation, as well as of federal civil rights agencies, Detlefsen attempts to show that administration efforts to implement reform were thwarted by judges and federal officers committed to a "civil rights ideology."

One could take issue with the thesis of this book. It is debatable that Ronald Reagan "failed" to change civil rights policy. This would only be true if one used the absolute criteria posed here. Moreover, it reflects a judgment that is focused solely on the short term. The author notes that three important cases in 1989 clearly indicate the current Supreme Court's willingness to undermine affirmative action policies. But even if one were to accept the author's focus, he is less than successful in making the case.

First, legal precedents constrain judicial decisionmaking. Although political scientists like to act as if judges have the power to do whatever they want, the limits placed on them by precedent are meaningful. As a result, judicial policies must change slowly. It also takes time to replace justices and judges on the bench with those who have the proper ideology. Despite a record number of appointments to the federal bench, Reagan could not change the federal bench overnight.

Second, despite the emphasis that Detlefsen places on the importance of a civil rights elite, his own account suggests that others outside that elite were either hostile, or uncommitted, to change. For example, he notes that efforts to abolish the use of quotas in hiring by the Civil Rights Commission were opposed by those in the business community. He also notes that those appointed to civil rights positions by the Reagan administration were often at odds on civil rights policy. Finally, Reagan himself was ambivalent, providing very little leadership on the issue.

Indeed, one of the interesting questions not addressed in this book is how these civil rights policies have managed to survive in the face of public hostility. The check on unpopular policies is not the courts but the voters, who presumably exercise their protest through the ballot box. Jesse Helms's 1988 campaign is only one piece of evidence that this issue is ripe for exploitation. To say that Reagan has failed is premature.

Washington University, St. Louis LIANE C. KOSAKI

No Escape: The Future of American Corrections. By John J. DiIulio, Jr. New York: Basic Books, 1991. 301p. $22.95.

Courts, Corrections, and the Constitution. Edited by John J. DiIulio, Jr. New York: Oxford University Press, 1990. 338p. $32.50.

The Scale of Imprisonment. By Franklin E. Zimring and Gordon Hawkins. Chicago: University of Chicago Press, 1991. 244p. $29.95.

Jails: Reform and the New Generation Philosophy. By Linda L. Zupan. Cincinnati: Anderson Publishing Co., 1991. 201p. $14.95 paper.

Each of the works reviewed here agrees that America's prisons and jails are overcrowded. Taken as a whole, these works demonstrate the tremendous legal, administrative, and political consequences of this obvious fact. In *The Scale of Imprisonment* Zimring and Hawkins look for causes of this phenomenon. DiIulio, in *No Escape*, surveys the broad landscape of prison overcrowding and offers suggestions for its management, while in *Courts, Corrections, and the Constitution* he pulls together authors of diverse points of view to assess in detail the impacts of court intervention into prison management. Finally, Zupan, in *Jails*, presents a very narrow focus on the issue of overcrowding by examining one heralded solution to its effects, if not its causes.

The implications of the problem lead Zimring and Hawkins to search for causes. In an impressive review of a broad and diverse literature, they suggest that no real causes have been found. In reviewing social and historical process theories, they find no consistent evidence pointing to a cause. Nor do intentional policy choices, such as mandatory sentencing, have a causal effect. They similarly report that neither variations in crime rates nor various policy changes have an impact on prison population sizes. Public opinion, politics, economic forces, and even drug convictions are ruled out. The absence of discoverable causes compares well with DiIulio's premise that there is "no escape" from the problem. Moreover, Zimring and Hawkins are in agreement with DiIulio that remedy plans have not succeeded as policy. While DiIulio does find that intensive supervision probation is effective, it can only be used for a small portion of the incarcerated. He bases this on the assumption that 95% of the prison population is "dangerous" and belongs there. This assumption is not uniformly shared by policymakers or analysts.

Zimring and Hawkins, painting with an even broader brush, suggest that none of the remedy policies has succeeded in lowering prison populations. Despite this minor disagreement, DiIulio argues that the only way to solve the problem is to manage it. Overcrowding can be overcome. The difference between prisons, then, is the management. Accordingly, he offers several principles by which to manage. However, these principles run the risk of being seen as "proverbs" or the "one best way" to manage a prison. As the Zimring and Hawkins data suggest and the case studies in *Courts, Corrections, and the Constitution* demonstrate, the contours of the problem change with the context. It is difficult, therefore, to imagine one set of immutable principles.

DiIulio also argues the need to limit court intervention in prison administration. While he concludes of his edited book that on balance, the results of judicial intervention in prison administration have been positive, he presents as a basic principle for judges that they must *decide things and order things incrementally* (borrowing from Lindblom's analysis). DiIulio reaches this conclusion by analyzing case studies, arguing that too vast changes made too quickly result in chaos. He takes a decidedly harsher tone in *No Escape*.

Other contributors present a more mixed view. Three views of the Texas prison case (Ruiz) suggest that while there was some disorder and violence, this was overcome and the results of intervention were more or less positive. The case of the New York City jails (particularly the Tombs) is also seen as a success, though Zupan does not even address the role of the court as part of the success. Rather, her work suggests that the new generation philosophy was responsible for the limited success.

The authors in *Courts, Corrections, and the Constitution* provide a glimpse of a taxonomy of variables and a framework within which to view judicial intervention.

Feeley and Hanson discuss the impacts of reforms and the institutional and organizational atmosphere resulting from intervention. To this we may add the state's (Useem) and city's (Storey) political climate. Differences between trial court and appeals court case handling are of significance (Talarico and Chilton), as are differences between the power of federal and state judges (Useem). The impact of due process reforms has discoverable limits (Rhine), but policy change can be implemented with a "fixer" and a powerful judge (Storey).

What solutions to prison overcrowding seem to emerge from this broad landscape of contingent variables? Zupan suggests that the new generation of jail design and management represents one possibility. Her evidence is mixed on the capacity of design and management reform to ameliorate the spin-off problems (violence and staff burnout). However, she argues that this solution is understudied and offers some hope of alleviating the symptoms if it cannot cure.

Zimring and Hawkins suggest discovering a political economy of incarceration. For them, the problem is the "free lunch" that incarceration offers in the public choice model. The sentencing authorities (the courts) do not have any costs associated with sentencing. Hence, other variables cause prison population fluctuations independent of the true costs of incarceration.

DiIulio offers a partial resolution of cost problems by suggesting the nationalization of the prisons, having them administered by the Federal Bureau of Prisons but paid for by the states. He also suggests, as more achievable solutions, that guidance be offered to judges to make them more effective. This is important if, as Bradley's contribution suggests, Republican judicial appointees are no more and no less likely to be interventionist-minded. DiIulio also suggests that good management would go a long way to solving most of the difficulties associated with overcrowding.

It is interesting to reflect that the same argument can be made regarding judicial intervention. As Ekland-Olson and Martin note in their contribution, the pattern of resistance by management to court orders in Texas was the true source of harm. If so, then intervention can be dealt with, as well. While none of the authors suggests an analogy, the model of bankruptcy courts for managing the assets of major corporations comes to mind. Rarely is this economic bankruptcy model questioned. Yet in a case of "constitutional bankruptcy," the federal courts are seen as incapable. The evidence weighs in favor of intervention based on these readings.

Each of these works will be useful to a broad variety of scholars. *Courts, Corrections, and the Constitution* will be of great interest to students of policy, implementation, state and local politics, public administration, public law, and criminal justice. It is well written and exceptionally well organized. In contrast, *No Escape* is more likely to be useful to public administration and criminal justice scholars. Like the edited volume, it is highly detailed but less balanced in its presentation of the evidence. Both are brilliantly written and are highly recommended.

The Scale of Imprisonment has an exceptionally well designed literature review of interest to public policy, criminal justice, and public law scholars. Its careful review, analysis, and critique of research is stimulating and inventive. This book, too, is highly recommended as well.

Jails is far narrower in scope than the other three. For those with a specific interest in jail design and problems related to management at the micro level, this work will prove useful. It does not address issues most policy or implementation scholars would find of interest. However, students of criminal justice policy may find this useful, depending on their focus. It relies heavily on the social psychology of jails and is accordingly devoid of interesting content for most political scientists or students of public administration. It is a thorough and painstaking assessment of this new theory in jail design and management. While it ignores many interesting theoretical issues (e.g., it fails to demonstrate empirically any essential link or dependency between design and administration), it is recommended for those with interests in this limited area.

California State University, Chico GREGORY D. RUSSELL

The Constitution and the American Presidency. Edited by Martin L. Fausold and Alan Shank. Albany: State University of New York Press, 1991. 323p. $49.50 cloth, 16.95 paper.

Martin Fausold and Alan Shank adumbrate the theoretical issues on which this symposium volume's arguments converge. Following their remarks are essays by historians and political scientists that explore and explicate various aspects of the constitutional development of the presidency.

The historical selections in part 1 focus on how disparate presidents from Thomas Jefferson to Harry Truman augmented the constitutional authority of the executive. Ralph Ketcham suggests that Jefferson's presidency combined the Aristotelian ideal of good government and a classical republican emphasis on civic virtue with creative executive leadership. Robert Remini illustrates how Jackson, through his domination of other institutions of government and his control of the nation's agenda, "reconstituted" the office of the presidency. Michael Les Benedict shows that Lincoln, even though he established "precedents for presidential power that remain potent today," was ultimately cognizant that "inherent presidential powers" neither constricted nor superseded the constitutional authority of Congress (pp. 55–56). William Harbaugh links Theodore Roosevelt's expansion of presidential prerogatives with the unprecedented growth and concentration of "private economic power" that obtained in the latter part of the nineteenth century. In his portrait of the presidency, Ellis Hawley describes and contrasts Hoover's and FDR's policy responses to the Great Depression. He also considers how the resolution of constitutional questions precipitated by FDR's "managerial solution" for this economic crisis affected the constitutional evolution of the presidency. Hawley concludes that the strictures imposed by the constitution were "altered to accommodate" the "administrative state" and the "modern presidency" inaugurated by the New Deal (p. 104). Finally, Donald McCoy illustrates how Truman's broad interpretation and vigorous assertion and defense of his constitutional authority facilitated the extension of presidential power.

Written by political scientists, the chapters in part 2 underscore salient constitutional issues and questions provoked by the accretion of presidential power in domestic and foreign affairs over the last half-century. Louis Fisher argues that the Budget Act of 1974 entails complex legislative procedures that favor presidential control and decrease presidential responsibility in the budgetary process. He contends that this piece of legislation has culminated in irresponsible budgetary decisions that the Gramm–Rudman–Hollings Act of 1985 could not control and that an item veto will not correct. Louis Koenig describes in detail how even though the founding fathers "left open the question of which branch was to have the decisive voice in foreign affairs," presidents have expanded their powers to realize considerable autonomy in the foreign policy arena (p. 173). Similarly, Richard Pious provides ample evidence to show that the War Powers Resolution of 1973 has failed to curtail presidential autonomy in military affairs. Donald Robinson emphasizes that the presidency, as it has developed over last half-century, "poses a dire threat to constitutional government" in the United States (p. 212). To meet that threat, he proffers several constitutional reforms designed to restore salutary restraints on executive power. In a carefully reasoned argument, Jeffrey Tulis challenges the conventional viewpoint concerning the scope of executive power implied by the "original constitution." Applying a novel and rather evocative interpretation of the constitution, he advances the thesis that the political order engendered by the New Deal did not represent a revolution in constitutional principles. Ultimately, he argues, large government and the modern presidency are really "implicit in commitments ratified two centuries ago" (p. 134).

Theodore Lowi's afterword provides a lucid and exceptionally trenchant analysis of the constitutional development of the presidency as it is portrayed in the other essays. He underscores the analytical weaknesses of the "heroic/developmental theory" of the presidency that links the discrete historical studies of this book. He also repudiates the ideological uses of the heroic model exemplified in both liberal and neoconservative analyses of the presidency. He opts, instead, for a "radical constitutionalist position." In Lowi's view, this model is constitutional because it accentuates the primacy of the separation of powers and checks-and-balances and "radical because it is willing to sustain some losses, if losses are necessary to sustain constitutionalism" (p. 241).

Overall, the essays in this work represent informed, judicious, and even provocative treatments of constitutional issues and questions involving the presidency. This collection, however, is not devoid of minor shortcomings. Some of the historical studies tend to be a bit too episodic. Thus, these essays do not always make clear how the precedents established by the presidents they depict shaped and constrained choices and actions of succeeding executives. In addition, concern for the president's expanded role in foreign and military affairs dominates the political science component of this work. More attention could have been given to the constitutional constraints that encumber executive action in the domestic arena. Still, this work offers a broad range of splendid studies that will be extremely valuable for anyone interested in understanding the constitutional authority of the executive.

Weber State University GARY LEE MALECHA

Water Resources Management: In Search of an Environmental Ethic. By David Lewis Feldman. Baltimore: Johns Hopkins University Press: 1991. 247p. $38.50.

Thirst for Growth: Water Agencies as Hidden Government in California. By Robert Gottlieb and Margaret FitzSimmons. Tucson: University of Arizona Press, 1991. 286p. $35.00.

These are two very different books that take a somewhat similar case study approach to their subject. In *Thirst for Growth*, Gottlieb and FitzSimmons have provided a very thorough and readable history, description, and analysis of six major water agencies in southern California. The first five chapters provide histories of the Metropolitan Water District of Southern California, the San Diego County Water Authority, the Upper San Gabriel Valley Municipal Water District, the City of Burbank Public Service Department, the Imperial Valley Irrigation District, and the Kern County Water Agency. Chapters 6–10 examine various political and management issues, including the isolation of water managers from public scrutiny; the role of consultants, the press, and community groups in water agency management; the increasing role of water markets in water management; and various management techniques designed to improve "efficiency," such as conjunctive use and peak pricing.

Gottlieb and FitzSimmons describe a water industry preoccupied with growth and expansion and insulated from the public (which it cares very little about) that pursues policies of supply augmentation, rather than the politically riskier demand reduction (a strategy consistent with the growth orientation of the agencies) and is more concerned with keeping costs down (e.g., by inexpensive and less efficient purification methods) than with pursuing policies that best ensure the public health.

This is a good book. Those unfamiliar with water management issues (e.g., water marketability and conjunctive use) will find the book a valuable introduction to these and other subjects and the problems of implementing new ideas in water management. All readers will enjoy the case studies of the agencies examined and the descriptions of the bureaucratic and institutional cultures that have played such an important role in the development of southern California.

The authors argue that their work uniquely contributes to our understanding of water management by showing that "local agencies arise out of local initiatives, rather than from a general logic of water development" and that "it is local initiatives, not more widespread constituencies, that stimulate and support federal investment" (p. xviii). Although it does not detract from the value of their work, the authors are not the first to make this observation.

In the concluding chapter, the authors make several suggestions for reform. These include increased public input and public participation, a move away from the growth orientation of agencies in the not-too-distant future, more attention by water agencies to equity and quality of life issues, and redirection of agency efforts away from expansion and toward a greater focus on water quality. These are all good ideas. It would seem that given the geography of southern California, a shift away from a growth orientation is inevitable and that water quality problems will necessarily demand more attention. As concerns quality-of-life issues, it would seem that the growth orientation of the agencies in this study have already largely dictated the terms of that debate in southern California.

David Lewis Feldman also uses case studies in *Water Resources Management*, examining the Garrison Diversion Project in North Dakota and the Blue Ridge Pump Storage Project in North Carolina and Virginia. Feldman suggests we adopt a new environmental ethic in water resources management, as well as in natural resources management generally. Using a combination of regional resource management, increased public participation, and acceptance of the assumptions of deep ecology, Feldman posits an ethical system for human intervention with nature. Beginning with the premise that "the conferring of water rights upon a few groups and the placement of authority for policy in the hands of an engineering elite have produced a water policy both undemocratic and unresponsive to environmental concern" (p. 54). Feldman proposes that water be managed through a system of regional, self-sustaining authorities (based on the hydrogeographic regions developed by the Water Resources Council). Management would be guided by equal access and appreciation for the interdependency of natural resources (i.e., ecosystem balance), historical patterns of development, and Pareto optimality. Water managers would allocate water on a principal of "universal equity," taking into consideration (among other things) the rights of other living creatures. Such a system would "require self-restraint and an abandonment of self-interest" (p. 197).

Although I enjoyed the book and would use it in my water resources seminar to stimulate discussion, I was sometimes left wondering where the water resources world that I was familiar with fit in. Although most people would agree that water resources policy has often been skewed to the advantage of certain well-financed or well-organized interests and has, at times, ignored the concerns of the less powerful, and although it is not difficult to agree that the ditch-dam-and-dike mentality of water resources development in the past has ignored ecosystem sensitivity and more benign ways of managing our environment, some of Feldman's solutions strike me as politically unfeasible and others as practically undesirable. Concerning the former, I see little chance that the current political actors in the water policy arena would be willing to turn their powers over to regional authorities. I suspect we may have to deal with the current set of water management institutional arrangements for the foreseeable future. On the other hand, there is some evidence that water resource agencies are becoming more cognizant of the externalities associated with their activities. We can largely thank the National Environmental Policy Act and environmental organizations for that.

Both of these books are valuable contributions to the water resources management literature. Feldman has applied certain aspects of political theory and environmental ethics to water resources management in a new way. The result is an interesting resource management ethic that, while not entirely new, deserves additional attention. Gottlieb and FitzSimmons give us excellent case studies, which provide insight into the history and politics of regional water agencies. Although they do different things in quite different ways, I would recommend them both.

Northern Arizona University ZACHARY A. SMITH

The Ideology of Apolitical Politics: Elite Lawyers' Response to the Legitimation Crisis of American Capitalism, 1870–1920. By James C. Foster. New York: Garland Publishing, 1990. 167p. $44.00.

The Justice Broker: Lawyers and Ordinary Litigation. By Herbert M. Kritzer. New York: Oxford University Press, 1990. 233p. $35.00.

Social critics and political scientists have long been fascinated by American lawyers and their role in public life. Tocqueville's references to the "aristocratic propensities" of lawyers in America and his observation that "in the United States . . . lawyers, as a body, form the most powerful, if not the only, counterpoise to the democratic element" are cited in study after study in which scholars have probed for the sources of the remarkable prestige and influence enjoyed by lawyers in this country. Political scientists, in particular, have been curious to know (1) why lawyers behave as they do and (2) why they have had such a profound impact on American politics and society.

Herbert M. Kritzer's *Justice Broker* is a careful study of the day-to-day work of attorneys who handle ordinary (i.e., small) civil cases. It is mainly concerned with question 1 and is based on data collected by the Civil Litigation Research Project (commissioned by the Department of Justice) in 1979–80. Kritzer contends that our understanding of the work of ordinary civil litigators can be improved by adding a new theoretical perspective to the traditional image of the lawyer as a "professional." This new perspective conceives of the lawyer as a "broker"—a person hired to act as an intermediary between his client and the courts or, more generally, between his client and all components of our system of justice. The image of the lawyer as a broker contrasts with the traditional image of the lawyer "as an alter ego [of the client], which is the notion typically used when talking about the professional" (p. 12).

The applicability of the brokerage model to the study of lawyers was suggested by its usefulness in the study of local politicians who function as intermediaries between their constituents and larger social and political systems. However, the model does not fully describe the relationship between lawyer and client. Kritzer confesses that at the outset of his study, he expected the image of the lawyer as broker to supplant the image of the lawyer as professional but that his research persuaded him that neither image by itself was sufficient to account for the experience of ordinary civil litigators. Hence, he did not discard the professionalism framework that has dominated modern sociological analysis of the legal profession but decided, instead, to supplement (or "overlay") this framework with the brokerage framework. His claim, which he defends persuasively, is that exclusive reliance on either framework will produce a distorted picture of what ordinary civil litigators ordinarily do.

Kritzer's data reveal that a typical civil litigator's professional life is complicated by a wide variety of concerns, commitments, and responsibilities. Although the lawyer often furthers the client's interests in a more or less direct way, various aspects of the lawyer's self-interest frequently come into tension, or even conflict, with the client's interests. In such circumstances, the image of the lawyer as the alter ego of the client is obviously inadequate. Kritzer concludes that "in some ways the broker image is better for explaining the work of lawyers in ordinary litigation, while in other ways the professional image is better" (p. 19).

Kritzer's study is a model of competent social-scientific inquiry into a complex social phenomenon. It is dispassionate, thorough, analytically rigorous, and judicious in generalizing and drawing conclusions from the data. Although Kritzer is methodologically self-conscious, he lets the data call the tune and refrains from manipulating his findings to vindicate a preordained theoretical agenda. In reading *The Justice Broker*, one senses (and shares) the author's satisfaction in achieving a more complete understanding of the complicated lives and roles of ordinary civil litigators.

Kritzer's valuable book stands in sharp contrast with James C. Foster's polemic against American lawyers. *The Ideology of Apolitical Politics* is as tendentious as Kritzer's study is fair-minded. In Foster's book (which is mainly concerned with question 2) a political agenda, not the data, calls the tune; and it is a strain that we have all heard before.

Foster's agenda is avowedly neo-Marxist. He means to prove that "in this society, *lawyers are the liberal guardians of capitalism*" (p. 1; emphasis original). He understands his project as "a reworking of Tocqueville's views on the legal profession" (p. 3).

Foster draws heavily on influential works on the legal profession by Arnold Paul and Jerold Auerbach, as well as on the writings of scholars associated with the critical legal studies movement. The general theoretical framework is informed by Gramsci's understanding of "hegemony," Therborn's notion of "mechanisms of subjection," and Habermas' idea of the "legitimation crisis." Unfortunately, Foster makes virtually no effort to engage serious critics of the thinkers on whom he relies. The result is a string of undefended assertions supported by little more than citations to the works of controversial leftist academics.

Here is Foster's basic story. American capitalism, which he defines as "a social formation characterized by private ownership and/or control of productive property used for profit" (p. 1), is legitimated by liberalism, which he understands as a legalistic political ideology that portrays human relationships fundamentally in terms of rights and rules. The cherished ideal of the rule of law is merely a mask for the rule of lawyers. Lawyers are "mongers of rights and rules" and "professional liberals largely in capitalists' employ" (p. 146). Law in liberal society is nothing more than politics masquerading as something apolitical. The prestige and influence of lawyers derives from their strategic role in serving the interests of their capitalist masters by sustaining "the ideology of apolitical politics."

Foster seeks to persuade readers of the accuracy of this story by recounting the activities of "conservative" and "progressive" lawyers from 1870 to 1920, "a period of social discord 'close to outright class conflict'" (p. 9 quoting Arnold Paul). During this period, the American Bar Association was organized, together with bar associations in 47 states (p. 45). Foster lays special stress on speeches given by elite lawyers at meetings of these associations throughout the country. He purports to show how "legal conservatives" (fundamentally "Hobbesian" in grounding civil government in rules based on fear of political chaos) and "legal progressives" (essentially "Lockean" in rejecting political absolutism and basing government on rights) sought in different ways to legitimate capitalism by sustaining the ideology of apolitical politics.

If one imagines reading a study of atheists by Rev. Jerry Falwell, one will have a pretty good idea of the tone of Foster's analysis of legal elites. Having already decided what they were *really* up to, Foster works and reworks the data to make them validate his neo-Marxist story.

Foster maintains that by sustaining the ideology of apolitical politics, lawyers continue to obscure the vision of the great majority of Americans who are victims of capitalist exploitation. He admits that his analysis does not square with "the way most Americans perceive their lives," a regrettable fact that merely confirms for him Therborn's thesis that "ideology is a powerful mechanism for maintaining power" (p. 149). Hence, Foster declares that "if Americans could see existing social reality clearly we might freely imagine a more just . . . alternative and act to bring it into being" (ibid.). Recognizing that "such a development would spell the end of the(ir) world, lawyers, as well as the capitalist interests they represent, . . . employ all [the] ideological mechanisms of subjection to ensure capitalism's survival" (ibid.).

Someone interested in understanding contemporary lawyers and their work is likely to learn a great deal from Kritzer's study of civil litigators. Someone interested in a sober critique of the role of lawyers in American political and economic life is unlikely to gain many insights from Foster's book, though as an example of fierce polemical writing, it might be hard to beat—at least until Rev. Jerry Falwell writes that book about atheists.

Princeton University ROBERT P. GEORGE

Islands under Siege: National Parks and the Politics of External Threats. By John C. Freemuth. Lawrence: University of Kansas Press, 1990. 200p. $25.00.

Islands under Siege focuses on two case studies of Washington-level policymaking as it affects national parks. One concerns legislation and regulations affecting tar sands leasing in and near the Glen Canyon National Recreation Area, the other the evolution of efforts under the Air Quality Act to protect parks from visibility impairment.

The analysis is low-key, appropriately viewed as storytelling, and contains none of the didactic partisanship that mars much popular and professional discussion of national park issues. Freemuth has also abjured the theoretical and methodological heavy-handedness that render much social science literature impenetrable to citizens, undergraduates, and resource managers. That is the book's greatest virtue—the narratives introduce the general reader to tar sands leasing and visibility issues, both in general and as they affect national parks. Freemuth's careful and informative introduction to visibility is illustrative: it weaves technical concepts such as scatter, absorption, and their relationship to particular airborne chemical pollutants with the political jargon of SIPs, BARTs, integral vistas, and regional haze. Moreover, the author has included several useful appendixes, including a summary of major National Park Service legislation and a brief introduction to different types of units in the national park system. The book is a solid contender for a resource-policy-, legislative-process-, public-administration-, or interest-group-oriented reading list and a valuable point of entry for non–political scientists interested in either the cases or the national parks.

Because of that clear priority, the volume may disappoint specialists. It is not ground-breaking for either the discipline or for students of the public lands. In the absence of clearly stated and consistently treated hypotheses, the author's discussion lacks edge and the conclusions are somewhat uninteresting. For instance, "It is obvious that both the political process and chance played a much larger role in this issue than rational land-use planning" (p. 49). Again, "It is hardly surprising, then, that the agency in question will interpret the law to correspond to that agency's norms and values" (p. 55). Finally, "Once an area is under Park Service administration, concern for overlooks and park visitation can drop off the preservationists' agenda." (p. 83).

Freemuth alludes repeatedly to issues of major theoretical and practical significance. The role of science in public management is traditional fare for the political scientist and has been the focus of public concern, most intensely in the national park context, since the Yellowstone and Yosemite fires and Alston Chase's popular volume, *Playing God in Yellowstone*. Freemuth does not invoke the literature or enter into the debate. Similarly, he does not build on the organizational behavior literature, which features such standard gems as Herbert Kaufman's *Forest Ranger* and Ashley Schiff's *Fire and Water* to compare the Park Service to the U.S. Forest Service. Nor does he comment on Bureau of Land Management–Park Service relations, central in the tar sands case, or Environmental Protection Agency–Park Service relations, central to the visibility issue. Both the extensive literature, which includes the more recent work of John Baden, Terry Anderson, and Gary Liebcap (all with a public lands focus) and the comparisons inherent in the two cases are missed opportunities in Freemuth's slim volume. And although he seems primarily concerned with the quality of interest group advocacy in behalf of national parks ("If we can come together in our love for our parks, then the external threats issue might be resolved," p. 143), he does not talk very much about interest groups either in theory or in context.

A sharper focus and analytical framework would not have threatened the author's commitment to entry-level comprehension. And although the appendixes include a brief and useful discussion of some of the classic studies in Park Service policy (Runte's *National Parks*, Foresta's *America's National Parks and Their Keepers*, and Ise's much earlier *Our National Park Policy*), he has omitted essential references such as Sax's *Mountains without Handrails* and has not made use of this literature in structuring his discussion. The volume suffers, one might argue, from being a bit of an island itself.

As a result, numerous central issues that ought to be raised are not. Most importantly for public lands scholars, the context of Freemuth's islands–external threats treatment of the parks is rich with significant questions. The island concept featured in the title is a familiar but not obviously appropriate metaphor for a national park. Similarly, the notion of external threats is problematic. In theory, since the classic case of *Camfield* v. *United States* 1897 and certainly since Joseph Sax penned his major article "Helpless Giants" in a 1976 issue of *Michigan Law Review*, there has been clear basis for asserting that the federal government has considerable authority to protect its property from threats arising on adjacent lands. It is not clear, then, why Freemuth insists without discussion both that the external threats issue "is a relatively new one" and that the federal Government has "no regulatory control over development outside [park] boundaries" (p. xii, 9).

Finally, along with discussing approaches to problems such as visual impairment, Freemuth ought to have helped us ponder why external threats have become the focus of so much recent discussion of national park degradation. Many would assert that the external threats over which, ostensibly, the Park Service has inadequate control are not a patch on the problems that the agency itself causes with proconcession and pro-tourist-oriented policies and/or bad science. Clearly, external threats and islands in themselves are political notions that ought to be analyzed, rather than taken at face value. Loving national parks does not necessarily make one love the National Park Service—or should not. That said, Freemuth's carefully told and easily penetrated stories are still a good place to begin an evaluation of the issues he presents.

University of California, Berkeley SALLY K. FAIRFAX

The Boundary Waters Canoe Area: Wilderness Values and Motorized Recreation. By James N. Gladden. Ames: Iowa State University Press, 1990. 166p. $19.95.

The Quetico–Superior boundary waters region provides the canoeist a peak wilderness experience paddling pristine lakes and portaging canoes and "duluth" packs between lakes—plus some great fishing. This region consists of two major management units, the Boundary Waters Canoe Area (BWCA) within the Superior National Forest in northeast Minnesota and Quetico Provincial Park across the international boundary in western Ontario, totaling 2.225 million wilderness acres, plus the new, small Voyageurs National Park at the western end of the BWCA. As a great reserve that is unique in North America, the Quetico-Superior area is a mecca for eastern and midwestern wilderness lovers and serious canoeists.

James Gladden describes the battles over management of the BWCA portion of the area. The 1964 Wilderness Act specifically incorporated the BWCA as a flagship unit of the national wilderness preservation system. However, the BWCA's status in the system, since its reservation as a roadless area in 1926, represented an anomaly because there was a long history of motor use in the area. From 1956, local residents and resort operators fought to retain rights to continue preexisting motorized uses (motor boats and later snowmobiles) and regarded provisions in the Wilderness Act as guarantees of their uses.

For preservationists, motor use within the Quetico–Superior area is anathema. The minimum criterion of a statutory wilderness in the United States is that it must be roadless precisely because motorized transportation is antithetical to the concept of wilderness. Gladden points out, interestingly, that wilderness canoeists's notions of technological purity are somewhat self-deceptive. Motors are deemed a philosophically incorrect intrusion of modern technology into the solitude of the park. Yet the equipment used by the wilderness enthusiast is hardly the primitive gear of the French voyageurs. In any case, preservationists and local promoter interests fought a running battle into the 1980s over the size of the BWCA management unit that would be closed to motorized use. The pivotal point in this story was the passage of the BWCA Wilderness Act of 1978.

Gladden's case study focuses on two major interest group protagonists—the preservationist Friends of the Boundary Waters Wilderness and the promoter Boundary Waters Conservation Alliance—and the difference in the resource use values of the two groups. The former are characterized as romantic preservationists for whom BWCA wilderness canoeing is a physical test in an area that provides none of the supports of modern technological society. On the other side, the locals in Ely, Minnesota regard nearby federal lands as their land, to be used as they wish and with whatever energy-consuming conveniences they prefer, and resent the intrusion of "outsiders," federal bureaucrats, and "politicians" (i.e., Congress) into their freedom of choice.

Unfortunately, Gladden's book is only a mildly interesting case study, though one that would be useful to scholars of natural resources policy and students who wish to study decision making in a major public lands issue. A case study can certainly be the foundation of a fine book. But a good case study should include one or more special traits. The case study could provide factual examples for some general theory of political processes. The classic example of this genre is Graham Allison's *Essence of Decision* (1970). Or a good case study could provide an insightful description of the particular policy process or use state-of-the-art social science methods to document its argument. Finally, a truly fine case study should be strongly written.

By these tests, Gladden's book is unsatisfying. First, his stab at a philosophic framework is a flop—a reference to an article by Riley Dunlap from the 1980 *American Behavioral Sciences* that borrows Thomas Kuhn's metaphor of paradigms to restate concepts of biocentricity and anthropocentricity that are well understood to be fundamental to philosophies of resources conservation dating back to the nineteenth century. Beyond that, Gladden's analysis of the values of the protagonists is limited to minor caveats: the prowilderness coalition ignored the history of motor use prior to the Wilderness Act, for example, while the locals had no conception of motors' disturbance of the wilderness experience of canoeists and cross-country skiers.

Second, Gladden's description of the BWCA policy process oversimplifies the protagonists by focusing exclusively on the pro- and antimotor coalitions and adopts a simplistic definition of pluralist politics as the result of the competing preferences of the two coalitions. Gladden particularly skips over the trials of the Forest Service, the agency that manages the BWCA. The BWCA Plan was the Forest Service's most controversial land use plan of the 1970s, with 1,013 comment letters—a foretaste of the mailbag wars of 1980s national forest planning. Moreover, Gladden's descriptions of both the legislative history of the 1978 BWCA Act and the subsequent litigation are one-dimensional and flat.

Third, the study presents generalizations about the values of the two coalitions but no systematic evidence or analysis about the character of the constituent groups. What, in particular, is the variance among group members' attitudes, and how well do the range of members' attitudes and beliefs fit the generalizations Gladden attributes to the coalitions? Or what are the sources of group members' values? Wilderness lovers seem to come from a different sociopolitical subculture than snowmobilers and motorboat users.

Finally, Gladden's book does not read well. I can assure readers that participants in local public lands politics are passionate, articulate, fascinating folks. But Gladden's wooden prose just does not convey either the fire in the belly or the sophistication with which the participants fight out the issues. Nor, ultimately, does it do justice to the beauty and majesty of the Quetico-Superior region itself.

Quetico Provincial Park PAUL J. CULHANE

The Cynical Society. By Jeffrey C. Goldfarb. Chicago: University of Chicago Press, 1991. 299p. $22.50.

In *The Cynical Society*, Jeffrey Goldfarb argues that "the single most pressing challenge facing American democracy today is widespread cynicism" (p. 1). He believes that the political parties, the mass media and culture industries, the educational system, and movements of both the Left and the Right contribute to this problem. Yet Goldfarb also believes that there is potential for a genuinely democratic culture in the United States, and he calls for a renewal of democracy as a counter to the cynical society which has emerged in recent years. In carrying through this project, Goldfarb offers challenging theoretical perspectives on cynicism, a sharp critique of contemporary politics and culture, and a wealth of illuminating insights into our contemporary malaise.

In an illuminating discussion of cynicism as a philosophy, Goldfarb indicates that it originated in Greek philosophy with Diogenes who utilized cynicism as an instrument of critique of the dominant society from the position of the powerless. Later cynicism, however, developed into a philosophy of resignation and became increasingly cynical in the modern sense of the word. In the contemporary era, Goldfarb believes that cynicism has functioned largely as an instrument to defame criticism of the existing order and programs for social change. In addition, he believes that it has become a mass attitude that compounds powerlessness and resignation to the existing order. For Goldfarb, cynicism is thus a block to progressive social change both on the intellectual and mass-political level.

Like Daniel Bell, Goldfarb appears to blame the ills of the present age on the sphere of culture. He writes, "The major problem of mass society is its cynical culture" (p. 51). Further, "we must recognize that mass culture is a response to the underlying tensions in democratic ideals and practices" (ibid). Goldfarb's position, however, is more subtle than those neo-conservatives who would blame the ills of the polity on contemporary culture alone, while defending the economic and political system. Goldfarb recognizes that it is the control of both politics and culture by a corporate elite which uses the state and culture to advance its own economic interests which is the problem with contemporary culture and politics and not merely contemporary culture. Yet unlike Marxists who simply posit that the capitalist class controls culture and politics and leave it at that, Goldfarb analyzes the complex web of mediations that constitutes contemporary culture as cynical. He notes that corporate control of culture puts commercial interests before the interests of enlightenment, the promoting of democratic debate, or producing a quality culture. But he recognizes that there are contradictory impulses within contemporary culture between democratic and commercial forces and that, often, genuinely democratic and commercial considerations are interwoven.

For example, Goldfarb interprets the rise of public education, the development of public lecture circuits in the 19th century, the mass publication of books, and public libraries as democratic developments that attempted to educate the public so that individuals could participate within public life. But he also notes that commercial concerns compromised the public lecture circuits once agents began organizing lecture tours (pp. 56ff) and that publishing has also become overly commercial in the present age, putting profits before public enlightenment and reasoned debate (pp. 58ff). Above all, however, Goldfarb sees the centrality in contemporary culture of a commercial media system as the key constituent of a cynical culture and impediment to development of a democratic culture.

Yet Goldfarb does not analyze the role of media ownership or the political economy of the media as contributing to the increasing commercialization of media culture. During the 1980s, in the wake of Reagan's deregulation policies, major corporations took over the mass media and have arguably used them to promote a conservative agenda. Goldfarb, however, does not discuss the consequences of corporations like GE taking over NBC or the Time-Warner merger which, in my view, pose threats to democracy both from excessive concentration of state, corporate, and media power and by failing to adequately inform the public or debate issues of national concern.

Most of Goldfarb's book, however, is an ideological critique of contemporary culture and politics rather than a structural analysis of the U.S. polity and economy. In general, Goldfarb is very good in his cultural analysis and ideological critique and weaker in analyzing economic, political, and social mediations that constitute the current crisis of democracy in the United States and what he calls the rise of cynical culture. Goldfarb provides an adequate but not particularly novel analysis of the rise of the New Left and New Right and criticizes both for falling prey to ideology and using cynical appeals to attract the masses to their ideological politics. His strategy is to argue that both the New Left and the New Right are equally cynical in their politics, as are contemporary Republicans and Democrats. But this ploy collapses Bush and Dukakis, Republicans and Democrats, and Left and Right into one cynical essence, overstating similarities while occluding differences. A more adequate analysis, therefore, would analyze the specific modes of cynicism and attempt to carry through a more concrete and specific discrimination between the dominant modes of politics and discourse today.

Such an analysis, I believe, would reveal Bush and the Republicans to be especially cynical in their politics. Bush is arguably one of the most cynical politicians that has ever emerged into prominence, sacrificing a career of moderate Republican centrism to the hard-right ideology of the Reagan era and then fluctuating from moderate to rightist conservative positions during his own presidency, failing to articulate any coherent position. Indeed, there are deep suspicions that Bush has consistently lied to cover over his activities during the Iran/Contra affair and other scandals of the Reagan era and I would argue that Bush regularly engaged in lies and hypocrisy throughout the prelude, duration, and aftermath of both Iran/Contra and the Gulf war, two of the most cynical episodes in U.S. political history. In addition, Bush's promotion of Clarence Thomas as the best man for a position on the Supreme Court and his naming his crony Robert Gates as the best man to head the CIA despite a dismal record of failure during his tenure in the CIA represent rather extreme cynicism. Thus I believe that Bush requires a separate study of his politics of cynicism and that Goldfarb's attempt to balance discussion of Bush's cynicism with the Democratic Party's cynicism represents apolitical liberalism which fails to make key discriminations.

The University of Texas at Austin DOUGLAS KELLNER

A General Theory of Crime. By Michael R. Gottfredson and Travis Hirschi. Stanford: Stanford University Press, 1990. 297p. $39.50 cloth, $12.95 paper.

Theories about crime abound. There are too many to summarize here, but they may be characterized in terms of their political values. Conservatives tend to see the cause of crime as rooted in individual deficiencies. Wilson and Herrnstein's *Crime and Human Nature* (1985), for example, argues that although there is no criminal gene, traits that are to some degree inheritable, such as intelligence and temperament, affect the likelihood that people will engage in crime. Liberals, on the other hand, tend to believe that social and economic factors are the major causes of crime. Gottfredson and Hirschi fall into the former camp, for they argue that people who lack self-control are likely to commit crime. These people tend to have unstable marriages, friendships, and job profiles and few skills and to be self-centered and insensitive to the suffering of others. The major cause of low self-control, they say, is ineffective child rearing. Parental concern for the welfare or behavior of the child is a necessary condition for successful child rearing. When this is absent, then the child will have low self-control.

The authors refute sociological and psychological explanations of crime and argue that self-control is "the only enduring personal characteristic predictive of criminal (and related) behavior, regardless of other dimensions of their personality" (p. 111). Because low self-control develops early in life (before age three), neither rehabilitation nor incapacitation policies will work. Instead, they assert, "effective and efficient crime prevention that produces enduring consequences would thus focus on parents or adults with responsibilities for child rearing" (p. 269).

Gottfredson and Hirschi devote a large portion of their book to refuting biological, psychological, economic, and sociological positivism (part 2) and also trying to discount some of the things that seem to contradict their theory (part 3). For example, crime is committed primarily by males. Does this mean that they have lower self-control than females? (chap. 6). Also, the incidence of crime peaks in the age range of 15–18 and then declines. Does this mean that somehow people are able to learn self-control as they mature? This would run counter to their notion that self-control is learned very early in life. Most difficult for their theory is white-collar crime—crime committed by people who have gained high positions by exhibiting a great deal of self-control. How can this be explained by their theory? Their answer is that their theory predicts low rates of offending by those in high positions. They reject research that says white collar and corporate crime are extensive and cost the nation far more in money and lives than street crime. In this regard they agree with J. Q. Wilson's *Thinking about Crime*, which also argues that street crime is the real threat to the social contract. The credible evidence, the authors say, shows that white-collar crime does not cost huge sums of money. But it is hard to reconcile this with recent events such as the savings-and-loan fraud, Wall street insider trading and the Housing and Urban Development scandals.

Their theory, they say, is "meant to explain all crimes, at all times, and for that matter, many forms of behavior that are not sanctioned by the state" (p. 117). This, of course, is an extremely ambitious claim. It is almost an attempt to create a theory about life itself, since crime is such an inherent and important part of life. No wonder, then, that they have to perform wonderous mental gymnastics in order to explain away much of the research that is incompatible with their theory.

Although theories about crime that see its cause as a deficiency in individuals, rather than society, are becoming popular, particularly among conservatives, such theories have a long history. But I agree with Emile Durkheim's *Rules of Sociological Method* (1964) that crime is socially constructed and cannot be blamed on an imperfectly formed mind.

Arizona State University Dennis Palumbo

Transforming Free Speech: The Ambiguous Legacy of Civil Libertarianism. By Mark A. Graber. Berkeley: University of California Press, 1991. 347p. $39.95.

Today, Americans believe that freedom of speech is a basic human right that plays an essential role in the functioning of our democratic political processes. Free speech has not always enjoyed such a highly preferred place in our society. There have been periods of considerable repression of political dissidents. But (insists Graber) most Americans—including most academics—wrongly assume that the strong contemporary position is based upon a uniform tradition of free speech interpretation dating back to the framing of the First Amendment.

In fact, Graber argues, there have been two differing constitutional conceptions of free speech. A conservative interpretation dominated American discourse from the Civil War until World War I. For the remainder of the twentieth century, a liberal interpretation based upon the functional relationship of free speech to democratic government has been prevalent. *The Transformation of Free Speech* explicates that paradigmatic shift and discusses its consequences.

What Graber labels the conservative libertarian defense of free speech was based upon natural rights theory. Most political scientists are aware how natural rights theory was used to create a constitutional order that provided a strong protection for private property. Graber emphasizes how that same matrix of ideas could encompass free speech. A theory of inalienable human rights, Graber shows, was used by some conservative commentators to argue on behalf of the free speech rights of political dissidents against the nationalistic claims surrounding World War I.

At first, the liberal, social science paradigm offered little comfort to civil-libertarians. When industrialization reshaped the socioeconomic infrastructure of American society, intellectuals found it necessary to talk about the need to reconcile competing group interests, rather than the need to protect individual rights. In championing the right of the people's elected representatives to reconcile competing interests, they had no rational basis for immunizing free speech from the instrumental calculus. This led to a series of judicial standards for interpreting the First Amendment, particularly during the early Cold War period, which hardly seemed to recognize the existence of a constitutional right of free speech. Justice Felix Frankfurter's opinion in *Dennis* v. *United States* 1951 is the leading example of this failure of liberal theory.

Gradually a persuasive defense of free speech emerged. First, following John Stuart Mill, civil-libertarians maintained that free speech is essential for formulating sound social policy. Second, they argued that their understanding of the relationship between free speech and self-government was embedded in the First Amendment and uniformally subscribed to by all major American thinkers since the founding. Both elements of this argument enabled liberals to urge strong judicial protection for all political ideas in the name of American democracy.

Graber—perhaps unduly—maintains that Zachariah Chaffe, Jr. was the galvanizing figure in this development. Be that as it may, Graber clearly shows how the new construct, while historically inaccurate, became the prevailing constitutional interpretation. Given the structure of his argument, he is also able to note its fragility. *Brandenburg* v. *Ohio* 1969 is the leading case. Yet as Justice Douglas's concurrence noted, the Court's opinion was still another balance test; and in times of great stress, judges have generally permitted asserted national interests to outweigh a dissident's rights to free speech. The current conservative Supreme Court's instinctive deference to executive power and its strong concern for social order also makes the survival of the prevailing civil-libertarian interpretation problematic.

More important to Graber is the fact that Chafee and his disciples sharply distinguished between political speech (which warranted strong judicial protection) and socioeconomic matters (which were subject to all regulations governmental officials believed reasonable). Political ideas and campaigns sponsored by the wealthy were thereby immunized: "Elected officials have been constitutionally barred from equalizing participation in the marketplace of ideas by limiting the economic resources persons can bring into that forum"; and "disparate economic holdings are affecting the capacity of elected officials to deliver programs responsive to the needs and sentiments of a majority of Americans" (pp. 186, 189).

Here Graber's failure to tie his constitutional arguments more directly to political theory creates problems. The role of money in contemporary American politics does not pose an insuperable problem for civil-libertarians working within their own paradigm. That is why some commentators have begun to address the issue (as Graber recognizes, pp. 213–15). Rather, Americans are constrained by their more general beliefs. The Western liberal tradition has never favored an egalitarian approach to property. Plainly, the conservative adherents of natural rights theory would never have argued for a limit on property rights in order to advance free speech. Similarly, liberal adherents of the welfare state are willing to regulate the economic market but not to supplant it. They, too, believe that people are entitled to benefit from the fruits of their labor.

Graber's concluding recommendations are based upon Michael Walzer's *Sphere's of Justice*, but they could just as readily be grafted upon the existing paradigm. They are eminently sensible. A liberal electoral majority might well enact them and a liberal Supreme Court might well sustain them. But it is doubtful whether that would satisfy Graber. He apparently desires a truly major transformation, one based upon communitarian theory. Graber neglects to explain why, after more than two hundred years, Americans are likely to abandon a political order based on liberal political theory for a communitarian vision of society. American conservatives and liberals both appear to believe that Madison's arguments in *Federalist* 10 are conclusive on this point.

This book reflects the best aspects of contemporary free speech scholarship, and the worst. Graber provides a persuasive account of how civil-libertarians came to adopt the prevailing constitutional paradigm and an illuminating description of that construct's strengths and weaknesses. Unfortunately, Graber's discussion of free speech conceptions is not closely tied to more general democratic theories. The absence of that broader political theory framework reduces both the force of Graber's criticisms of the prevailing liberal paradigm and of his admittedly modest prescriptive suggestions.

State University of New York, Albany MARTIN EDELMAN

Impossible Jobs in Public Management. Edited by Erwin C. Hargrove and John C. Glidewell. Lawrence: University Press of Kansas, 1990. 213p. $29.95 cloth, $14.95 paper.

Amid the swirling controversies over appropriate public responses to problems such as AIDS and the mentally ill homeless, psychologist Erwin Hargrove and political scientist John Glidewell offer a stunningly simple observation: some public tasks are impossible. The contributors to *Impossible Jobs in Public Management* show how and why jobs grow to become impossible. Yet theirs is not a counsel of despair; for the main focus of the book is to examine how top agency officials cope with impossible tasks. Much like the successful organizational innovators profiled in Lewis's *Public Entrepreneurship* (1980) and Doig and Hargrove's edited *Leadership and Innovation* (1990), officials who are "professional-political entrepreneurs" seem to stand a better chance in their struggles with so-called wicked policy problems.

The analysis proceeds in two steps. In part 1, Glidewell and Hargrove draw on both fields to explain why some public tasks become impossible and to sketch appropriate coping strategies for top agency officials. They introduce four dimensions along which public jobs can be arrayed. Jobs falling at the *difficult* end of all four continua are those classified as *impossible*.

Six impossible jobs are identified for detailed examination in part 2. DiIulio focuses on commissioners of corrections, Miller and Iscoe on state mental health commissioners, and Lynn on social welfare executives while Lawlor writes of the challenges the AIDS crisis poses for public health officials, and Byers examines the task of special masters in monitoring court-ordered prison reform.

These part 2 essays vary in the extent to which they rely on the conceptual discussion in part 1. Yet all do an excellent job of elaborating on the impossibility of the particular tasks they focus on. Less uniformly compelling are the analyses of, and prescriptions for, officials' coping strategies. Especially thoughtful and informed by the cases they examine are the DiIulio, Lawlor, and Lynn essays. In contrast, Miller and Iscoe conclude with a list of recommended strategies for state mental health commissioners that reminds one of the infamous "proverbs of administration." Still, none of the essays claims that there is one best way to cope with impossible tasks; and most underscore the importance of the fit between individual managers and the political and policy situations they confront.

Impossible Jobs in Public Management may be valued most because it so runs against the current of much contemporary public discourse. It is a useful corrective to the bureaucracy bashing that has grown in volume and intensity over the last two decades. The contributors argue convincingly that an important explanation for government ineffectiveness is that some policy tasks inexorably trigger irreconcilable controversy over the goals that should guide public response (e.g., Should imprisonment rehabilitate, deter, punish, or isolate criminals from society?). Meanwhile, these same tasks typically involve clients (convicted criminals, welfare mothers) whom citizens both understand as needy and perceive to be unworthy. Given weak policy technologies and the absence of acknowledged experts to guide policy, the political system veers almost drunkenly between commitments to, and attacks on, public action.

That top agency officials might have an impact in these difficult policy arenas would be a useful datum in ongoing debates over what public agencies at all levels of government can and should do. At this point, however, perhaps the major limitation of the analysis surfaces, namely, that *better* and *worse* coping by agency managers are not clearly defined. Possibly, "better" copers might be those who can foster some improvement in one or more relevant policy indicators. Yet if the task is indeed impossible, this is an inappropriate evaluative criterion, since either such improvement cannot be achieved, or else it will be merely transitory or produce unanticipated negative consequences, or it will reflect how little a manager could have accomplished. Ultimately, most of the contributors fall back on another standard of success—the length of tenure of the top executive. One who stays longer must be coping better than one whose term is shorter. Whether longer tenure is desirable from the standpoint of clients, constituents, or the political system as a whole is far less apparent. Indeed, many would view the acceptance of such a standard as part of the problem in public management.

Nonetheless, *Impossible Jobs in Public Management* is a useful and thought-provoking book. Its examination of specific policy areas offers compelling accounts of the conflicts and frustrations associated with many of the tasks public agencies undertake. Scholars of public bureaucracy may well lament the lack of systematic attention paid to internal agency dynamics, and agree with DiIulio that "broad contextual variables" are probably more important in explaining agency performance than is the top executive (quoted p. 68). Even so, Hargrove and Glidewell's argument for the key role of agency entrepreneurs is worth both careful consideration and further scrutiny.

Virginia Polytechnic Institute and State University KAREN M. HULT

Restructuring Justice: The Innovations of the Ninth Circuit and the Future of the Federal Courts. Edited by Arthur D. Hellman. Ithaca: Cornell University Press, 1991. 385p. $42.50 cloth, $13.95 paper.

Caseloads of the U.S. circuit courts of appeal have grown dramatically over the years. The traditional congressional response has been to add judgeships to the existent courts, rather than split the busiest circuits and allocate the new judgeships among them. But in 1975, the congressionally established Hruska Commission concluded that this traditional solution was growing less and less viable because at some point, ever larger appellate courts become unwieldy and undermine justice. (For example, inasmuch as the circuit courts, with rare exceptions, decide cases by randomly drawn, shifting panels of three, the possibility of contradictory rulings and precedents increases along with the total number of judges on each court.) The Hruska Commission thought that no circuit court should have more than nine judges. Beyond that point, the circuit should be split, with each of the new circuits having its own circuit court of appeals.

The commission recommended that Congress split the two largest circuits: the Ninth and the Fifth. Congress did divide the Fifth Circuit; but the Ninth, under the aggressive leadership of recently elevated Chief Judge James Browning, fought back and to this day remains intact (although bills to split the Ninth continue to be introduced). Browning's major strategy was to inaugurate a host of reforms designed to prove that despite its huge size, his court could surmount the problems thought by the commission to be inherent in large appellate courts.

This book enters the Hruska–Browning debate over the size of appellate courts. The Ninth Circuit, the book convincingly argues, is an especially appropriate research setting; for it is by far the largest of all the circuit courts, whether measured by caseload (about one-sixth of the total caseload of all 12 circuit courts), number of judges (more than three times as many as the U.S. Supreme Court), or the vastness of the far-flung area it serves (California, Oregon, Washington, Montana, Idaho, Arizona, Nevada, Alaska, Hawaii, and the territories of Guam and the Northern Mariana Islands). The focus is on the Browning era (1976–88); for that is when the most far-reaching reforms occurred (e.g., his attempt to minimize contradictory rulings by increasing communication among panels and by utilizing staff to spot possible contradictions before written opinions were finalized). The authors earn high marks for grounding their research in, and integrating it with, the substantial body of scholarly literature published prior to 1990.

The most consistent theme of *Restructuring Justice* is that the Browning reforms worked well and demonstrate that the Hruska Commission was wrong when it concluded that large appellate courts are necessarily plagued with problems of efficiency and fairness. In fact, the Hruska Commission's alternative of splitting large circuits is singled out for special criticism on the ground that that would only maximize contradictions in federal court precedents; for while even the largest circuit court can work to reduce contradictions among its own rulings, there is no similar coordination among the U.S. circuit courts of appeal (except, of course, for the relatively few cases of circuit conflict eventually resolved by the Supreme Court).

Although this book seems quite objective, I could not help but wonder about the possibility of subtle bias. The editor and several of the authors express great admiration for Judge Browning, to whom the book is dedicated "in recognition of his tireless service to the more effective administration of service." The volume was sponsored "to pay tribute to Judge Browning"; Browning and other Ninth Circuit judges "reviewed drafts for accuracy" (pp. xiv–xv). I should hasten to add that some of the authors present findings suggesting that the Browning court was, indeed, plagued with some of the problems spotlighted by the Hruska Commission.

The editor and several of the authors correctly note that this study has implications far beyond the Ninth Circuit. As the caseloads of both state and federal appellate courts continue to grow, the lessons learned in the Ninth Circuit may help Congress and state legislatures more wisely assess the merits of "circuit splitting" versus adding judges to the old courts. On balance, I think those lessons are well analyzed in *Restructuring Justice.*

Illinois State University GEORGE C. KISER

The Politics of Obscenity: Group Litigation in a Time of Legal Change. By Joseph F. Kobylka. Westport: Greenwood, 1991. 206p. $45.00.

When changes in the membership of the Supreme Court bring about a departure from established doctrine, how do groups active in litigation adapt? In answering this question, the author of the present study attempts to give some order to, and find a conceptual framework for, the study of group participation in litigation. The raw material he has selected for investigation is a list of 119 obscenity decisions handed down by the Supreme Court between 1957 and 1987.

Miller v. *California* 1973 was a watershed decision, marking a turn from the libertarian (or at least permissive) approach to sexually oriented material by the Warren Court. The Burger Court was willing to "leave the determination of obscenity to local authorities and thereby diminish the oversight role of the federal courts in this area of the law" (p. 7). The winners in obscenity decisions before *Miller* were organizations that (like the American Civil Liberties Union) championed unrestricted free speech. After *Miller* these organizations were on the defensive, and groups dedicated to the protection of public morals were in the ascendency. The author has hypothesized that the about-face in *Miller* should affect the litigative activities of groups and provide a basis for theorizing about the dynamics of group resort to the courts.

Kobylka criticizes the existing literature on group litigation on several grounds. It is "plagued by a lack of theoretical depth," focuses on disadvantaged group actors, does not examine behavior in times of legal flux, and "lacks an awareness of the dynamics . . . of that litigation over time." Isolated case studies "underplay the relationship between group behavior and the general context in which the groups operate" (p. 15).

Kobylka's investigation finds that 24 groups litigated obscenity questions during the 30 years covered by the study. The groups fell into two categories: *purposive* groups interested in public goals and *material* groups with an economic stake in affecting the outcome of the Court's work. Groups on both sides of the obscenity issue used various strategies (sponsorship of litigation, monetary support, and participation as *amici curiae*) to advance their interests. From an inspection of group activity, Kobylka found that the changing legal doctrine after *Miller* affected groups in different ways. Some, like the ACLU, decided to pursue other goals; others continued in much the same mode as before; and a third group, threatened by the decision, mobilized resources for damage control.

In the process of analyzing group responses to *Miller*, Kobylka has isolated primary and secondary factors that affect group decisions to litigate. Those internal to the group include associational type and intensity of commitment, relationship of leadership to members, and the nature and clarity of organizational goals. Those external to the group include the rules of the legal game (legal context), the nature of threats to each group, activities of other groups, and the general sociopolitical climate.

This is a carefully researched book that makes an important contribution to the literature in the field. However, the study does not attempt to quantify group use of different litigative strategies (one suspects that most of the participation is via amicus curiae briefs) and does not investigate activity in the lower appellate and trial courts. After *Miller*, trial court decisions became increasingly determinative of outcome. Kobylka's study is narrowly focused on cases decided at the Supreme Court level.

The notes attached to each chapter whet the reader's curiosity with tantalizing hints about political maneuvering and the details of specific campaigns, but the account leaves out much of the richness of this anecdotal material and concentrates on generalizing about the empirical data. One minor point that I wish had been followed up relates to the campaign against obscenity by radical feminist groups. There is no indication that antipornography feminists have attempted to influence policy by court action. Certainly, they comprise a politically powerless group. Existing theory would predict some such participation.

North Carolina State University EVA R. RUBIN

Supremely Political: The Role of Ideology and Presidential Management in Unsuccessful Supreme Court Nominations. By John Massaro. Albany: State University of New York Press, 1990. 272p. $49.50 cloth, $16.95 paper.

This book has redeeming features. Its detailed examination of presidential management in Supreme Court nominations in the modern era is a contribution to scholarly understanding of judicial politics. Massaro's "development of a common framework of analysis" (p. xi)—a focus on both the timing of High Court vacancies and party control of the U.S. Senate as indicators of presidential success or failure for nominations—will prove helpful to students of the judiciary.

His chapters on the unsuccessful Johnson–Nixon era nominations of Abe Fortas, Clement Haynsworth, and G. Harrold Carswell are useful, especially in his use of archival materials that offer some fresh insights into presidential and senatorial motivations at key stages of the confirmation process.

The work is flawed by its conclusion that when the Senate's October 1987 rejection of the Bork nomination is compared to the refusals to confirm Fortas, Haynsworth, and Carswell, "continuity rather than change in the factors accounting for the Senate's negative action is evident" (p. 191).

Massaro does not recognize that the Bork confirmation struggle represented both a qualitative and quantitative transformation in the politics of judicial confirmations. He completely neglects the impact of high-dollar paid political advertising (primarily, but not exclusively, financed by Norman Lear's controversial People for the American Way) on Bork's confirmation process.

The unprecedented mobilization of more than three hundred activist organizations of the Left in opposition to Bork is given passing reference here; and the underfunded late mobilization of several dozen conservative groups in support of the nominee is completely neglected (save for one reference on p. 186 to Coalitions for America, the umbrella organizing group that tried to rally the jurist's supporters). In short, the most distinctive features of the Bork confirmation battle get short shrift. This weakness is surprising, because Massaro's book appeared after the publication of Ethan Bronner's *Battle for Justice* (1989).

Even though Massaro's analysis of the Bork battle has some merit (he helps place the public pronouncements and private motivations of key senators in the context of other rejections of nominations in recent decades), there is a tendency to accept at face value the formal statements of key liberal senators.

This presents a particular problem for sound analysis of the Bork fight. Massaro accepts Howell Heflin's charges that Bork was extreme in his philosophy as a seriously held opinion. No mention is made of Heflin's scandalous use of religious grounds when he justified his vote to fundamentalist Alabamans. Similarly, moderate Democrat Dennis DeConcini's protestations about the nominee's sensitivity to women's rights are taken seriously although readers are not told about an explicit threat that DeConcini would face primary opposition in 1988 if he supported Bork's confirmation.

Massaro believes that Abe Fortas had helped to build "one of Washington, D.C.'s most prestigious law firms" and "had earned a reputation as one of the nation's finest and most powerful attorneys" (p. 219). Robert Bork's biographical sketch offers no such plaudits (p. 254). It is granted he was "one of the leading advocates of the doctrine of original intent" (p. 159). Republican senator Robert Packwood of Oregon, a leading advocate of abortion on demand, is a "conservative" (p. 88). The weaknesses of this study are revealed in such assumptions: Packwood is conservative, Fortas distinguished, and Bork extreme.

On the one hand, readers are instructed that the judicial mainstream is represented by a philosophy Americans have rejected for a quarter of a century. On the other hand, the judicial philosophy now verging on dominance at the U.S. Supreme Court is extreme; and those who have worked for its implementation in the judiciary are not worthy of mention. Massaro's study is recommended, but with the reservations noted.

Daily Oklahoman PATRICK B. MCGUIGAN

The Politics of Hispanic Education. By Kenneth J. Meier and Joseph Stewart, Jr. Albany: State University of New York Press, 1991. 275p. $19.95.

This is an important book offering a wealth of new information on Latino access to equal educational opportunities. Meier and Stewart analyze the quality of education for Latino children in 142 school districts with at least 5% Spanish-surname enrollment from 1971 to 1986. Most of the data for this study is taken from Mexican–American school districts, with some comparative data on Puerto Ricans in the Northeast and Cubans in Florida.

What emerges from *The Politics of Hispanic Education* is a pattern of systematic discrimination against Latino school children. While contemporary Latino students are no longer subject to the blatant abuses of racial segregation, they suffer a myriad of other malevolent practices. This process is what the authors call "second-generation" discrimination, policies that deny Latinos the educational opportunities available to Anglos. The authors argue that all that is negative about the American educational experience (tracking, expulsions, dropouts, corporal punishment, etc.) is associated with being Latino and that all the positive aspects (high school graduation, accelerated learning programs) is not.

One great strength of this work is that it will appeal to a wide range of readers. It addresses important issues in educational policy and fits in well with the literature on political mobilization, minority politics, and public policy. For example, the authors demonstrate the inherently political nature of the American educational process and how power relations between Anglos and Latinos determine the allocation of resources in a given school district. They document a clear association between Latino socioeconomic status, numbers, and system of local representation (ward vs. at-large) on the one hand and Latino representation on school boards, in administration, and on the school faculty on the other. These factors, in turn, affect Latino student's share of educational resources and their ultimate success in the school system.

Meier and Stewart also present two disturbing findings that will be of interest to policy analysts and political activists alike. First, bilingual education is not the panacea that some educational reformers thought it would be. Not only have bilingual education programs failed to boost Latino educational attainment, but under the control of biased school administrators, it has become yet another means of internally segregating Latino children. Moreover, the authors' data uncover a *negative* correlation between bilingual education programs and school success for Mexican–American and Puerto Rican students. These findings are all the more troubling because Latino public opinion fully supports the concept of bilingual education.

Second, Meier and Stewart find a positive relationship between the number of black students in a school district and Latino educational success. The authors hypothesize that as the black student body of a school district grows, they soon become the main focus of discriminatory policies. As Latino students cease to be the center of racial animosity, their educational attainment goes up. If this finding can be generalized to other situations, it implies an unfortunate predisposition for whites to make peace with Latinos in order to isolate blacks.

The book's major limitation is a data set that does not allow for an analysis of Latino educational success by generation. The literature on Latinos and mobility emphasizes that generation (e.g., first, second, third) is strongly and positively correlated with educational attainment, professional status, and income. Although Meier and Stewart document the disadvantages that all Latinos face in the school district, it is unclear how these variables impinge on the children of different-generation immigrants.

A second problem is that the book does not advance our theoretical understanding of racial politics and its underpinnings. The authors test a set of well-thought-out hypotheses with obvious theoretical implications, but do not create or build upon a dynamic model of racial politics. This is an unfortunate omission, given the authors' obvious command of the concepts and debates in the field. The first three chapters of *The Politics of Hispanic Education* consist of a detailed literature review of Latino educational politics. Finally, the authors' rich overview of the research on Latinos, politics, and education lies in stark contrast to their dry, methodical march through the data set. The ample use of jargon and the presentation of hundreds of statistical relations without a clear theoretical framework will make for slow reading for a non–quantitatively inclined audience.

Despite these problems, *The Politics of Hispanic Education* is essential reading for anyone interested in the politics of Latino education. Meier and Stewart ask the right questions and offer a well-grounded set of policy recommendations that might reverse the schools' failure to educate Latino children.

University of Wisconsin, Madison BENJAMIN MARQUEZ

The Political Failure of Employment Policy, 1945–1982. By Gary Mucciaroni. Pittsburgh: University of Pittsburgh Press, 1990. 317p. $34.95 cloth, $17.95 paper.

Employment policy, as Gary Mucciaroni observes in this provocative and important study, has been relegated to a marginal role in American economic policy. Formed largely as social policy with a welfare mission, it has featured modest goals for expanding employment, budgets too small to permit substantial accomplishment, and constituencies too narrow and weak to provide steady political support. It has been, in short, a political failure.

To document and explain this failure, Mucciaroni analyzes the development of employment policy in the post–World War II era—a period encompassing the Full Employment Act, the Manpower Development and Training Act, the Economic Opportunity Act, the Comprehensive Employment and Training Act (CETA), and the Job Training Partnership Act. On the basis of wide-ranging research, Mucciaroni discusses the historical and intellectual background of policy proposals, the debates and maneuvering in Congress, the implementation of programs, and their economic and political consequences. For comparative perspective, he offers a parallel (though much briefer) treatment of employment policy in Sweden.

A central argument motivating the political analysis is that employment policy was not intrinsically a feckless undertaking. With appropriate design and the necessary political capacity, it could have been far more activist, ambitious, and closely integrated with economic policy. Such a policy could have used a variety of micro interventions (e.g., labor market monitoring, training and retraining, relocation assistance, employment incentives, and direct job creation) to compensate for business cycles, facilitate adjustment to structural change, and sustain high levels of employment. In contrast with seeking high employment entirely through fiscal or monetary policy, it would not have had massive effects on aggregate demand or inflation. Mucciaroni presents public opinion data to show that an activist employment policy would have had public support and cites evaluation research and experience in Europe to suggest that it could have been workable.

In the central chapters of the book, Mucciaroni develops an explanation of why employment policy has remained peripheral. As an organizing framework, he proposes that the constraints on employment policy can be understood in terms of three sets of factors: interests, institutions, and ideas. He proceeds to develop penetrating, carefully reasoned explanations under each heading. In the concluding chapter, he recapitulates the analysis and distinguishes the separate roles of each set of factors.

The chief difficulty in the configuration of interests has been the political strength of business and the weakness of the liberal–labor coalition. Business influence has prevented a strong national commitment to full employment and has channeled fiscal management toward the use of tax cuts, as opposed to spending increases, for economic stimulation—a constraint that has inhibited the development of active employment policies.

The major institutional barriers to employment policy have been the fragmentation of decision structures and the weakness of central authorities. Following statist theorists like Krasner and Skocpol, Mucciaroni argues that fragmentation has permitted business to block ambitious employment policies and encouraged a proliferation of small, narrowly focused programs that have been impossible to implement effectively. Conflict between the president and Congress delayed the adoption of CETA and led to administrative arrangements that were incompatible with Congress's objectives. The complexities of intergovernmental implementation have led to politically disastrous breakdowns of control.

As for ideas, Mucciaroni observes that employment policy has inevitably been shaped by the predispositions of policymakers and the values and beliefs of the American public. Policy has been constrained by the ambivalence of Americans toward the values of equality of opportunity and equality of result. In particular, the ideological climate has militated against public creation of jobs and proposals to guarantee full employment.

Qualifying his arguments about interests, institutions, and ideas, Mucciaroni points out that the constraints they establish are not immutable; for they can be modified by political learning. Employment policy has evolved because of new information and changes in the views of economists and other elites about what policies are feasible. Indeed, Mucciaroni develops the interesting notion that a group's influence depends on its capacity to contribute to that learning.

Some liberals will probably complain that Mucciaroni overstates the failure of employment policy. Many conservatives will undoubtedly argue that he oversells its potential benefits. But there should be little debate about the cogency of his political analysis. The book is one of the best studies we have of the long-term development of a set of policies and a profound commentary on the limitations of the American welfare state.

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Urbana–Champaign
PAUL J. QUIRK

Money, Elections, and Democracy: Reforming Congressional Campaign Finance. Edited by Margaret Latus Nugent and John R. Johannes. Boulder: Westview, 1990. 294p. $31.95 paper.

Reforming our campaign finance system has proved to be an unusually difficult legislative task. There are sharp partisan differences based on competing policy objectives and divergent conceptions of what the real problems are. This collection of 15 essays attempts to clarify what is wrong with campaign finance law and to prescribe remedies for the ailments identified.

The volume includes articles by both scholars and practitioners, who analyze virtually all aspects of the congressional campaign finance system. The authors stick to the chore at hand, namely, to devise realistic reforms that warrant serious consideration by the Congress. Judged by this objective, *Money, Elections, and Democracy* is a clear success.

Although one wishes there had been more on the public and campaign finance, the two contributions that address this subject are first-rate. Ruth Jones's "Contributing as Participation" demonstrates that between 1980 and 1986 the percentage of Americans who donated to any candidate or campaign organization remained stable. There was, however, a significant drop in the proportion of donors contributing to political action committees, accompanied by a rise in the proportion of donors giving to political parties.

Frank Sorauf's "Public Opinion on Campaign Finance" is based on the author's close reading of all the polling data on public attitudes toward the campaign finance system. He concludes that "opinion on campaign finance is largely unorganized and unmobilized." For most Americans, campaign finance is an issue they find "difficult to define, difficult even to understand in all of its complexity" (p. 213). This goes a long way in explaining why, despite the hue-and-cry over campaign finance reform, Congress has not felt obligated to act.

The volume's primary shortcoming is more a reflection of the field than of the choices made by the editors. The common charge that interest groups wield undue influence because of their political action committees' donations is really given short shrift here. Only one of the political scientists in this work (Janet Grenzke) tackles this issue directly. Her article is an excellent review of the roll call analyses of political action committee influence on Congress. Many of these studies show no relationship between contributions and votes; others show a link between contributions and particular votes.

Although Grenzke herself suggests that the roll call studies fail to tell the whole story, the volume does not extend coverage of political action committee influence beyond her overview. There is no analysis, for example, of the connection between such contributions and legislators who intervene in administrative policymaking. It is likewise ignored how political action committee may influence committee agendas. In short, political scientists have a limited answer on this question because they have generally restricted themselves to a methodological approach that can be used to analyze only part of the legislative process.

While this collection stumbles on political action committee influence, it hits its stride when it looks at the other problems involving political action committees. Topics that are addressed include elections and incumbency (L. Sandy Maisel), accountability of contributions (David Adamany), independent expenditures (Candice Nelson), and loopholes in the Federal Election Campaign Act (Anne Bedlington). In another essay, Clyde Wilcox documents the growing trend toward personal political action committees in the Congress. By 1988, 45 members of Congress had their own political action committees. Wilcox argues that these have become important tools in leadership races and contests for committee and subcommittee chairs.

There are many other fine contributions to this volume, and the range of issues covered makes this an especially valuable collection. The authors' carefully constructed reform proposals offer a comprehensive set of alternatives to current campaign finance practices. Their recommendations as to how to fix our ailing system merit our attention.

Tufts University JEFFREY M. BERRY

Balancing the Federal Budget: The Cure for U.S. Wealth Dissipation. By Ernest J. Oppenheimer. New York: Pen and Podium, 1990. 181p. $20.00.

Ernest J. Oppenheimer is convinced that deficit spending has inflicted enormous harm on the U.S. economy, and this unusual book is his explanation of why and how the federal government should balance its budget. In arguing for a return to fiscal orthodoxy, Oppenheimer presents some provocative points; but neither his approach nor his style is likely to appeal to a scholarly audience. There is little in his presentation that qualifies as serious economic or political analysis; and his descriptions of budget policy are, unfortunately, superficial.

Oppenheimer's central argument is that deficit spending leads to wealth dissipation. As chronic deficits multiply and public debt levels climb, wealth is lost to foreign interests in the form of trade deficits, asset acquisitions, and investment income flows. The domestic economy, in Oppenheimer's view, is further enfeebled by the negative impact of deficit policy on interest rates, inflation, and saving. A credible case can be—and often has been—made for these assertions; but Oppenheimer is not interested in presenting evidence and analysis, nor does he cite sources to buttress his position. Instead, he repeatedly states his undoubtedly sincere belief that deficits and debt lead to economic, if not moral, impoverishment.

To be fair, sincerity of belief has always played a major role in the debate over deficits, as James D. Savage recently demonstrated in his excellent *Balanced Budgets and American Politics* (1988). Since waiting for an economic catastrophe to occur in order to prove deficits are bad does not make sense for even the most circumspect scholar, one should probably grant Oppenheimer some leeway in arguing this point. When he moves to solutions for the deficit predicament, however, one expects a reasonable familiarity with the spending and revenue trends that have created deficit problems. Oppenheimer is not interested in analyzing these trends but, rather, in advocating defense cuts and a gasoline tax.

According to Oppenheimer, the defense buildup of the 1980s remains the single largest cause of budget deficits and, even more alarming, subsidizes the foreign nations we defend, while leading inexorably to economic ruin (pp. 47, 91–94). In support of his first point about defense and deficits, Oppenheimer explains that the Reagan administration tried to pay for its defense program with domestic policy cuts, which Congress fortunately blocked. In fact, total discretionary spending (defense and nondefense) declined from nearly 11% of gross national product to just over 9% during the 1980s. Moreover, discretionary spending has lagged well behind economic growth for several decades; and the relative decline is projected to continue for quite some time, making the impact on deficits problematical.

Oppenheimer not only ignores this decline but argues that all trust fund surpluses (especially social security) be eliminated from any budget-balancing formula. There is no mention of the fact that there are more than 150 trust funds, nor is there any recognition that transfers from general funds explain a large portion of the projected $200-billion trust fund surplus in the mid-1990s. Oppenheimer fails to discuss the conventional view that the on-budget deficit is not meaningful from the standpoint of economic policy. Finally, there appears to be no comprehension whatever of the herculean policy cutbacks necessary to balance the budget under Oppenheimer's requirement.

Nevertheless, Oppenheimer proposes that defense cuts contribute approximately $100 billion to his $300-plus-billion deficit reduction program, with an additional $50 billion to be gained by eliminating unspecified waste in spending programs. The tax program he advocates includes sin taxes, luxury taxes, and assorted soak-the-rich initiatives. Its centerpiece, however, is a gasoline tax, which is slated to contribute some $80 billion in additional revenues. None of these figures is keyed to baselines of any sort. Instead, each is an extrapolation of current dollar levels.

It might well be, of course, that defense cuts and tax increases are desirable. It could even be that the gasoline tax Oppenheimer has advocated for many years "would have such a profoundly positive effect on our nation that its value would at least equal, if not surpass, that of the computer" (p. 111). In any case, it is difficult to evaluate propositions and claims of this sort when there is no evidence, analysis, or sustained argument to back them up.

Oppenheimer concludes several of his chapters with imaginary dialogues with a Democrat and a Republican, respectively. Oppenheimer uses these conversations to rebut his versions of partisan objections to deficit reduction, but partisanship is not the primary obstacle he faces. Rather, Oppenheimer will find it difficult to convince even dispassionate readers that his views are to be taken seriously. Those who are interested in an informed critical examination of U.S. budget policy must look elsewhere.

Southern Methodist University DENNIS S. IPPOLITO

The Humane Economy: Populism, Capitalism, and Democracy. By Norman Pollack. New Brunswick: Rutgers University Press, 1990. 215p. $40.00.

Who were the Populists? In Richard Hofstadter's pungent account, they were reactionary, intolerant, xenophobic, occasionally comical reformers searching for a lost agrarian age. Norman Pollack has long offered the opposite perspective that they were genuine grassroots democrats who offered America one last alternative to corporate domination. In *The Humane Economy* Pollack continues the argument.

Pollack explores Populist economics, then tries to look "through" the Populists for a glimpse at "the meaning of America" (p. ix). Between the lines, he defends the coherence and modernity of the reformers' vision.

In Pollack's view, the Populists were small rural capitalists. They celebrated property on the level of the individual home and community. (In his hands, these Midwestern yeomen look like direct descendants of the eighteenth-century civic republicans, though Pollack avoids making the connection.) The Populists spent their political passion attacking monopoly capital. They blamed the monopolists for, among other things, robbing the people of their livelihood, stealing their homes, forcing them into poverty, capturing their government, destroying their democracy and reducing the country to "a nation of hired men" (p. 16).

The Populist alternative would have grafted industry onto a democratic political base. They affirmed market competition; but they sought an independent state (independent of the monopolists) that would restrain capitalist activity, break up concentrations of power, and protect societal needs from economic activity. Wherever competition could not be restored, Populists would replace monopoly with state ownership. This, insists Pollack, is no machine-smashing outcry against modernity. On the contrary, Populists would embrace industrialization and labor-saving devices; but they would subordinate them to the general good, they would harness industrialism to create a "humane economy."

What happened? The usual answer is simple—rural reformers could not postpone modernity. Pollack scornfully attacks social scientists for accepting this deterministic view. Even words like *determinism* and *monopolism* have been replaced by the antiseptic *order* (or, he might have added, the relentlessly deterministic *rationalization*). History dismisses these losers and fears their commitment to democracy, says Pollack. As a result, we have forgotten that at the end of the nineteenth century, Americans were poised between corporate capitalism and the more humane Populist economy.

If so, the critical moment was a brief one. The Populists turned to politics (with the Omaha platform) in 1892; the movement collapsed four years later. Their swift demise gets to the heart of Pollack's contention. Once they entered the political fray, the Populists became steadily more radical till their reforms "threatened an irreversible conversion to radicalism" (p. 89). But these Midwestern democrats would not cross the line, would not allow their sectional and agrarian movement to become a national and class-based one. They would not violate the right of property. The limits of American populism lay, not in corporate opponents or irresistible economic forces but in the Lockean, liberal ideology that Pollack finds in the Populists' own heads.

Pollack's picture of the Populists is deft and nuanced. By insisting that we take their vision seriously by imagining what it would have looked like in practice, he creates a rich, compelling story. And a close look at the Populists does offer us glimpses of the genes that seem to get passed down the generations of American reform in the form of the search for an autonomous state free of special interests, a yearning for lost community (to be recaptured down in the grass roots), and (suicidally) a truncated political spectrum that limits the reforming imagination.

Still, political scientists are more likely to be tantalized than persuaded by the argument that the Populists had internalized an American political culture that checked their imagination and doomed their crusade. The trouble is that *The Humane Economy* does not explore competing explanations for the Populists' failure. It has little to say about the biases of the political economy or the reaction of the East Coast capitalists (or, for that matter, the Populist counterreaction). We get a deep, cultural, explanation before the more obvious alternatives are even considered. The book lacks the dialectic that Pollack tells us is missing from American history.

Finally, this may not be a book for readers meeting the Populists for the first time. The political history is sketched in a brief and perfunctory way. And the writing is tangled. But Pollack's passion for this movement gets him, and us, inside the Populist vision. The result is a fine, ultimately mournful account of one of the most important defeats in American political development.

Brown University JAMES A. MORONE

The Fourth Estate and the Constitution: Freedom of the Press in America. By Lucas A. Powe, Jr. Berkeley: University of California Press, 1991. 357p. $29.95.

This book, written by a law professor, is meant for other law professors. It contains little that is political and even less that is scientific. The author, without much explanation or justification, organizes his book around *New York Times v. Sullivan* (1964), the landmark decision that constitutionalized the law of libel. His rationale seems to be that "no other period manifested a tradition of protecting dissent better than this short, but remarkable, era" of the seven years from 1964 to 1971 (pp. 20–21). Fortunately, Powe does not limit his treatment to only these years; he surveys, instead, the whole of American history.

The problem, rather, is a degree of topical selectivity that leaves huge gaps in coverage. In addition to libel, he provides the reader with chapter-length treatments of prior restraint, access to sources and information, and media monopoly. These may well be, as Powe alleges, the most contentious contemporary legal press issues. Assuredly, they are not the only ones. Exacerbating matters is a focus restricted to the Constitution, ignoring statutory developments. The result is an intellectually unsatisfying treatment of press freedom. Dissatisfaction obtains, in part because coverage is unsystematic and in part because the author sharply distinguishes freedom of the press from freedom of speech specifically and from freedom of communication generally.

For example, although the author notes in passing the importance of subsequent punishment (p. 171), clear-and-present-danger receives but two pages of discussion, with an additional two given to *Brandenburg v. Ohio* (1969), the authoritative statement of the clear-and-present-danger doctrine (pp. 71–72, 95–96). Although *Brandenburg* was decided during the author's key seven-year period, it concerned verbal communication, not the printed word.

Even the first third of the book, which provides the background of press freedom, is haphazard. Powe begins with a telling description of what may well be the most outrageous instance of suppression of political dissent in American history, the saga of a U.S. senator and Colorado publisher whose punishment for a wholly truthful statement was upheld by the U.S. Supreme Court in *Patterson v. Colorado* (1907) on the grounds that the First Amendment simply does not prohibit the subsequent punishment of communication. The remainder of the introductory material bounds across well-traveled terrain: Peter Zenger's case, the Revolutionary period, the Alien and Sedition Acts, and World War I.

In the final third of the book, Powe presents two "models" that "promise some descriptive and considerable prescriptive accuracy": the public's right to know and the "fourth estate," meaning press autonomy (p. 233). Needless to say, neither is a model in any political or scientific sense.

Powe's book is not totally bereft of a political orientation. His treatment of the politics of *Miami Herald v. Tornillo* (1979), in which the Court validated press autonomy, is exceptionally well done. Because Powe is a legal realist, the tendentiousness that characterizes so many doctrinal works is pleasantly absent. But true to the disjointed treatment he otherwise accords his subject, he does not allow his realism to override his partiality for the press. Thus, his flippant criticism of the secrecy of the Court's conference (p. 196) better serves an editorial page than a scholarly work.

Michigan State University HAROLD J. SPAETH

Critical Pluralism, Democratic Performance, and Community Power. By Paul Schumaker. Lawrence: University Press of Kansas, 1991. 258p. $29.95.

Critical Pluralism, Democratic Performance, and Community Power is an in-depth study of power, influence, and policy making in Lawrence, Kansas, and the degree to which the policymaking process of the community is consistent with the normative ideals of pluralist democracy. Specifically, these ideals, nicely elaborated by Schumaker, include responsible representation, complex equality, and principle–policy congruence.

At the outset, Schumaker provides the reader with an excessively brief theoretical overview of the elitist, pluralist, economic, and regime interpretations of community power, devoid of much specific reference to earlier inquiries. This material is followed by a socioeconomic profile of Lawrence and an overview of its council–manager (mislabeled *commission–manager*) form of government, with a five-member council elected at-large on a nonpartisan basis.

In conducting his investigation, Schumaker employed a well-conceived, complementary, two-fold methodology. First, to secure an understanding of the underlying political culture of Lawrence, citizen support for conservatism and liberalism, and nine pairs of competing political principles (e.g., neighborhood rights versus property rights), Schumaker resorted to public opinion data garnered from interviews with 239 active political participants and a sample of 1,017 citizens. Not surprisingly, the citizens of Lawrence are about equally divided in terms of conservatism and liberalism; and tenets associated with the council–manager plan (e.g., the availability of good public services and "businesslike," efficient, and economical government) enjoy widespread support in the community.

Second, Schumaker, utilizing the comparative issues approach, discerned the participants involved in the resolution of 29 policy issues that confronted Lawrence in the time frame 1977–87. These issues, each of which the author describes in a nicely crafted mini-case study, involved a wide array of matters pertaining to the democratic process, economic growth, social control, the provision of services, and tax distribution.

Schumaker's central finding is clear and to the point: "The ideals of representative democracy are reasonably well attained in Lawrence; the community has been able to resolve issues without the maladies of external, elite, bureaucratic, or minority domination" (p. 173)—to wit, not even a ghost of a power elite exists in Lawrence. Instead, a sort of sloppy political pluralism prevails in the community, where policy issues are often decided in a rough, consensual, pragmatic sort of way, rather than on the basis of political principles.

Although Schumaker makes an impressive case that pluralist democracy is alive and well in Lawrence, he readily admits that some segments of the community benefit more than others from the local political system: "Issues are usually resolved in ways that do favor relatively advantaged participants—especially the middle class, Country Clubbers, Seniors and Veterans, Hometowners, conservative Republicans, Market Providers, Managerialists, members of the Growth Machine" (p. 201).

Critical Pluralism, Democratic Performance, and Community Power is a solid study that makes a significant contribution to our understanding of community political power and democratic political theory. It should be read by all serious students of community power. However, given Schumaker's penchant for political theorizing (especially his defense of pluralism), his volume could have been strengthened by incorporating the findings of earlier relevant contributions. Concluding analytical commentary does not need to be limited to a discourse based on the empirical findings of a single community.

Virginia Commonwealth University NELSON WIKSTROM

The NSC Staff: Counseling the Council. By Christopher C. Shoemaker. Boulder: Westview, 1991. 152p. $29.95.

Christopher C. Shoemaker, a former military officer who served on the National Security Council (NSC) staff under both Carter and Reagan, has written a useful overview of the history and functions of the NSC staff. This book begins with a historical review of the growing importance of the NSC throughout the cold war period. The author then turns to an examination of the various functions that the NSC has been asked to serve in recent administrations. He emphasizes, in particular, several relatively noncontroversial functions, such as administration of national security policy, interagency coordination, and advising the president on issues of foreign policy. He contrasts these with several more controversial functions, including crisis management, policy formulation, and (finally) policy advocacy.

Shoemaker argues convincingly that the strengths of the NSC derive from its standing bureaucratically independent of the preferences of any single agency and from its presidential perspective—the fact that it reflects in its composition and functioning the priorities and policy perspectives of the president, rather than the more parochial interests of any single bureaucratic agency. He then argues that this function reached its height during the Carter administration when Zbigniew Brzezinski was national security advisor, a role that largely deteriorated during the Reagan administration, when the NSC was downgraded and the advisor's position went through numerous hands.

Shoemaker suggests that this decline in central coordination of national security policy in the Reagan years was largely responsible for fragmented foreign policy decision making, of which the Iran–Contra fiasco was the most visible example. By contrast, he has a much more positive view of the functioning of national security policy in the last two years of the Carter administration; and he generally downplays the problems encountered in those years in the handling of the Iran hostage crisis, the SALT II treaty, and the Soviet invasion of Afghanistan. Indeed, most of the major foreign policy accomplishments of the Carter administration seemed to come in its early years before, as he puts it, "Brzezinski and the NSC Staff finally wrested control of the national security system" (p. 67). Thus, his prevailing view that the prominence of the NSC in the policymaking process seems to promote success of national security policy hardly seems to be born out by the actual experience of the Carter years.

The book concludes with an analysis of the changing national security environment (especially the shift from a military emphasis to a focus on issues such as economics, terrorism, the war on drugs, and ecological issues) and the implications of this changing environment for the structure of the NSC. He also notes the growing role of Congress and public opinion in the national security decision-making process. While these observations are generally appropriate, the suggestions offered are largely obvious and not particularly innovative. His major prescriptive recommendations include strengthening the role of the national security advisor, restoring the influence lost during the Reagan years, and adding staff positions to the NSC to deal with such issues as economics, low-intensity conflict, transnational issues (e.g., environment and population), mobilization and industry, and congressional relations. These sort of structural recommendations, however, fall far short of suggesting the kinds of conceptual changes that are required in dealing with national security in the post–cold war era, in which bipolarity has come to an end and American hegemony in the global system is also coming under increasing global challenge. These changes in how American policymakers conceive of national security are likely to be far more important than any simple bureaucratic reshuffling of the NSC staff in grappling with foreign policy making in the final years of the twentieth century.

In short, this book offers useful reading for all students of the U.S. national security decision-making process. To policymakers and public officials its offers some useful but not particularly innovative suggestions about how the NSC staff might be restructured. It acknowledges some of the important changes now underway in the international system, though largely failing to come to grips with the full range of rethinking that will be required to enable U.S. foreign and security policy to be more responsive to the rapidly changing demands of the new global environment.

Brown University P. TERRENCE HOPMANN

Congress Oversees the United States Intelligence Community, 1947–1989. By Frank J. Smist, Jr. Knoxville: University of Tennessee Press, 1990. 336p. $24.95 paper.

Frank J. Smist, Jr.'s interests in Congress and intelligence were stirred by his mentors James Lengle and Ray Cline. This his first book is a careful, scholarly history of congressional oversight of the intelligence community. Separate chapters are devoted to the Church committee, the Inouye, Bayh, and Goldwater Senate select committees, the Nedzi and Pike House committees, and the Boland committee in the House. A final chapter looks at the Senate Intelligence Committee under Republican senator David Durenberger of Minnesota; the House Intelligence Committee under Democratic representative Lee Hamilton of Indiana; the select committees on the Iran–Contra affair; and the Boren, Stokes, and Beilenson committees.

Smist's final chapter also contains his conclusions. Citing with approval the statement of a Senate Committee staffer that during the Iran–Contra affair, the Reagan administration had sought to escape the Constitution, Smist adds, "Such contempt for the American constitutional form of government was unprecedented."

"In the Iran–Contra Affair," Smist writes,

> President Reagan and his administration tried to do in secret what in a democracy they could not do in public. In showing contempt for the democratic process, President Reagan implemented policies that embarrassed the United States, damaged the national security, and undermined the American constitutional system. History must render a much harsher verdict on Mr. Reagan's job performance as president than the American people did in January, 1989.

In a summing up, Smist asserts that Congress must be "a full and equal partner of the president on intelligence." At the same time, he acknowledges that the executive branch unfairly refuses to trust the Congress to keep its secrets. Looking at the various proposals for improving Congress's carrying out of its intelligence oversight function, Smist comes down in favor of the present system of separate House and Senate committees. His other recommendations are unexceptionable: party leaders should appoint good people to the committees and find ways to reward them for a task that will not win them votes, the very best staff should be recruited, and the committees should demand a more meaningful role in decisions concerning covert action.

Only one of his recommendations should be challenged. Smist says that both the Senate and the House should reduce the number of their committees to bring "more order and efficiency to both chambers." One is reminded of the lifelong and totally fruitless labors of George Galloway. Galloway was also a political scientist and the principal staff member behind the herculean reorganization of Congress following World War II. The purpose of that reorganization was precisely to reduce the number of committees. But the need of members for a political platform, for extra staff, and for the other perks of chairpersons was overwhelming; and the proliferation of subcommittees began.

This also illustrates a point that should be made about the whole book. What is almost never discussed is the *politics* behind the moves and countermoves of the various members, committee chairs, and other actors in the story of congressional oversight of intelligence. Except for a short and very disapproving section on how Democratic senator Patrick Leahy of Vermont used his membership on the committee to get a lot of favorable media coverage during a tough reelection campaign, politics is hardly mentioned.

Columbia University ROGER HILSMAN

Learning Together: The History of Coeducation in American Public Schools. By David Tyack and Elisabeth Hansot. New Haven: Yale University Press and Russell Sage Foundation, 1990. 369p. $28.50.

The policy and practice of coeducation in public schools is a central issue in the continuing American experiment in democracy. The authors explore this issue in historical perspective, examining the evolving institution of public schools over the two-hundred-year history of the nation. In the process, they raise basic questions regarding the role of the schools in preparing boys and girls for assuming the roles of responsible citizenship in a democratic society.

The organizing concept for the book is an innovative exploration of the treatment of gender as a design for institutional form and practice in public education. The authors carefully trace the decision processes that led to the education of girls in colonial America and the nascent republic and document the economic and social bases of decisions by local school boards resulting in the acceptance of coeducation as the dominant educational policy in community schools by the mid nineteenth century. Coeducation, in turn, prepared educated women who were effective teachers and accepted lower salaries than men. To economy-minded local school boards, this practical alternative served as the basis for the transition of public school teaching from a predominantly male, to a predominantly female, profession in the early twentieth century. To modern feminists, the practice appears as an early basis for differential treatment by gender in employment conditions. To educated women in the decades of the late nineteenth and early twentieth centuries, however, teaching offered a welcome and respected alternative to economic dependency. In a balanced and perceptive study, the authors trace the dynamics of the policy over two centuries, from its inception in colonial New England through its acceptance as the dominant educational practice and the backlash of conservative groups to the concerns of modern feminists today.

In method of analysis, the book combines historical inquiry with public policy concepts and sources. The authors use historical photographs in a novel way to document gender practices in public schools over time. Taken for other purposes, the photographs provide unobtrusive evidence of gender as an organizing concept in institutional practice. The photographs document the conscious evolution of coeducation in classroom activities, and, interestingly, reflect the self-selection of same-sex groupings by school children in unstructured play. The contrast indicates the importance of coeducation as an institutional form that differed from the dominant social attitudes toward gender in the early nineteenth century and the significant shift in those attitudes over time. The use of photographs was even more compelling as evidence of the interaction of gender with race in public school practice. Although treated as a minor theme in the book, the historical reinforcement, in public education practices, of a strong role for black women but undervaluation of black men identifies gender differences in economic opportunities and social acceptance for blacks in the evolution from slavery to civil rights as a source of contemporary difficulties for black males in a society that still struggles with discriminatory practices.

The basic findings of the study are not new; but especially valuable is the closely reasoned interpretation given them in understanding the importance of gender policy in public education as a fundamental socializing experience for a democratic society. The authors establish five basic points in their argument. First, women are considered coequal partners with men in the New World enterprise of building a democratic republic. This position, articulated by Benjamin Rush and other founders of the republic, represents a conscious policy decision that distinguishes the new nation from its Old World heritage. Second, men and women had different but complementary roles in the New World; and they joined together in the larger enterprise of achieving the ideals of freedom, dignity, moral integrity, and continuing growth in the unfinished task of building the republic. Third, economic advantages reinforced the remarkably rapid transition to the dominant practice of hiring women as public school teachers, which simultaneously created a legitimate and public role for women in their communities. Fourth, early notions of gender differences in scholastic abilities are disproven and documented solidly over time. Finally, the public schools are established firmly as institutions that provide a primary socializing experience for the continuing evolution of a democratic society. This role offers both the opportunity and the obligation to create learning environments in which gender differences are acknowledged—but not turned into the basis for differences in legal or economic treatment. In light of the evidence presented by the authors, the same argument applies to racial differences.

In its research, documentation, and interpretation of findings, the book exemplifies a balanced treatment of gender issues in the evolution of public education in the U.S. Tyack and Hansot have produced an informed, carefully documented, and closely reasoned interpretation of the public schools as a formative social organization that shaped values and practices in U.S. society toward gender and race, melding visionary goals, economic interests, and social expectations in the bold effort to create a democratic society. The authors offer fresh insight into the evolving issues of gender and race that are of fundamental importance in the development of a democratic society and acknowledge the limitations in educational concept and practice that have subjected these issues to a full range of ideological distortions over the history of the republic. The book is remarkable for its balance, intelligence, wit, and reason in a probing inquiry into the history of coeducation as a continuing experiment in democratic practice.

University of Pittsburgh LOUISE K. COMFORT

The Rise of Candidate-centered Politics: Presidential Elections of the 1980s. By Martin P. Wattenberg. Cambridge: Harvard University Press, 1991. 186p. $24.95.

The American electoral system does not work the way it did within recent living memory. There are many who think that its performance, whether in democratic or governing terms, has significantly deteriorated. The changes have been well and truly rung in the professional literature on such themes as dealignment of the electorate, the emergence of candidate-dominated campaigns, the fracturing of electoral coalitions along office-specific lines, the emergence of divided government as a normal state of affairs for the first time in national history, and so on.

Thus, this little book by Martin P. Wattenberg may not appear at first glance to do more than restate emerging conventional wisdom along these lines of professional analysis. But *The Rise of Candidate-centered Politics* is not merely a new edition of the author's well-known *Decline of American Political Parties*. It attempts to construct a more general view of the current terrain and gives us some novel insights about it.

The overall story of dealignment is pretty extensively documented in this story. Some aspects of it are quite familiar, others not. Among the less obvious trends is a huge increase in the relative salience of economic factors in the open-ended candidate evaluations that voters make (p. 27). There was a clear jump-shift upward in the relative salience of economic factors between 1968 and 1972. Today, they enjoy a position of clear dominance over sociological and especially social–psychological factors in these evaluations. As the author points out, it makes sense for voters increasingly detached from traditional partisan identification and salience anchors to concentrate especially on the economic performance or promise of individual candidates.

It is also noteworthy that the popularity of presidential candidates in general has shown an aggregate tendency to decline over the past generation as candidate-centeredness in elections has emerged and become consolidated. Wattenberg gives us one more range of data that make it clear that in fact Ronald Reagan and his policies were anything but universally popular. Indeed, a review of both candidates in 1980 reveals that Carter actually came out ahead of Reagan, overall, along five dimensions of candidate popularity (p. 83). It was, in the end, economics that did Jimmy Carter in. For example, 1980 was the first presidential election year since 1932 in which there was negative growth in per capita disposable income.

By the same token, a prime key to Ronald Reagan's success in 1984 and George Bush's election in 1988 was a generally satisfactory macroeconomic performance—particularly notable in that it followed years of disorder and stress on both economic and other dimensions of our collective lives. As Wattenberg points out and many others have observed, voters in the aggregate are ends-oriented. They are largely indifferent to the means employed to reach a goal they desire (in this case, prosperity). We recall that the Democratic party suffered for decades from its association with the depression of 1893–97, just as the Republicans were to suffer, in their turn, for the "Hoover depression" that began in 1929. Now the wheel has turned again. A very clear implication of this study is that Republicans can continue to look forward to bright presidential election prospects until or unless major economic failure should once again coincide with their incumbency. In contrast with the past, of course, dealignment involves not Republican ascendancy across all of the policy institutions of government but, rather, divided government as a normal condition.

In one of the more interesting segments of this work, Wattenberg notes that there have been three major schools of voting analysis in modern times: the sociological, the social–psychological, and rational choice (associated with Berelson, the University of Michigan, and Downs, respectively). Analyzing the social sciences citation index over time, he notes that there has been a major change in citation dominance since the 1960s from the Michigan school to Downs, the latter enjoying impressive ascendancy by the 1980s (p. 19). While such evidence may be considered circumstantial at best in some quarters, the author's evaluation seems quite plausible. He argues that the transition away from party identification hegemony toward a candidate-centered electoral universe involves a more or less radical change in the field of observation. In this new empirical world, important professional problematics may be better or more credibly addressed in rational choice terms than through the social–psychological perspectives that once dominated voting analysis.

The Rise of Candidate-centered Politics impresses me as a credible and worthwhile study, but it is by no means beyond criticism. For one thing, I would have welcomed a somewhat broader and more extended discussion of macrosystemic consequences and implications as an appropriate capstone for the empirical presentations. For another, the author may be a bit too eager to argue that there has been no substantial change in the distribution of conservative, centrist, and liberal self-indentifications in the electorate over the past 20 years. This, of course, downplays the substantiality of Reagan's so-called mandate for policy revolution in the wake of the 1980 election. But even the data given in this work make it clear that the Right has gained quite substantially, while the center has shrunk and liberals hang on to their one-quarter share. Earlier NES data with a somewhat different measure strongly suggest that the 1960s distribution was a bell curve with the center dominant and the self-identified liberals somewhat outnumbering conservatives at each end. It would seem that the liberal erosion came first and was largely completed by 1972. Thereafter, it was the center that could not hold, so that by 1988 there is an unambiguous right-skewed curve replacing the old bell distribution. Party identification, a "lagging indicator," has been following suit since 1980, so that by 1991 we find a Republican plurality for the first time since modern surveys began asking identification questions in the 1930s. Despite the possible messiness of this indicator (For how many voters do such liberal–conservative categories really make sense?), there appears to be much more going on along such dimensions than the author says.

Finally, there is the question of placing this electoral and regime order in proper time boundaries. The rise of candidate-centered politics is not a phenomenon of the 1980s, although its consolidation certainly is. For all the vast importance of the Reaganite–Republican presidential transformation during the past decade, the system described in these pages came into being during and out of the vast political crisis of the late 1960s (which, interestingly, was not very visibly economics-driven). If one searches for major punctuational-change breakpoints in the author's long-term dealignment series, they can be found centering around 1966 for both split-ticket indicators and 1970 for the party identification and party attitude indicators. Other measures give similar breakpoint dates in system state. All of this constitutes evidence for a genuine critical realignment centering around 1968–72. Out of that crisis and realignment came the era of the "permanent campaign," the television-wired electorate, the dissolving of the class-centered glue of the Democratic presidential coalition, and candidate-centered politics—in short, the "sixth electoral era." Such scanning for periodization is not an exercise in quibbling but the first step toward placing the phenomena of the Reagan era in a more adequate historical (and thus analytical) context.

Nevertheless, much of the foregoing discussion implies a much longer work than the author chose to give us. Within its own terms and dimensions, *The Rise of Candidate-centered Politics* provides information and analysis that makes it a significant contribution to the professional literature on contemporary American electoral politics.

The University of Texas, Austin WALTER DEAN BURNHAM

Federal Preemption: The Silent Revolution. By Joseph F. Zimmerman. Ames: Iowa State University Press, 1991. 228p. $34.95.

The attention of political scientists to intergovernmental relations has risen and fallen with federal spending for grants. When outlays were high, the study of federalism was a booming business. When federal grant spending first stabilized and then dropped, many researchers lost interest. In this useful book, Joseph F. Zimmerman demonstrates that the flow of money has masked a far more important change in federal–state–local relations, with subtle shifts in power that have swamped programmatic effects in importance. A "silent revolution"—the rise of complex federal intrusions into what had previously been state and local domains—poses new problems of accountability and responsiveness.

At the center of this revolution is federal preemption, in which the federal government orders or induces the state and local governments to do what it wants done or in which the federal government itself moves onto turf previously dominated by subnational governments. Zimmerman identifies three kinds of preemptions: informal, total, and partial. *Informal* preemptions are the most widely recognized. The federal government awards grants conditional on prescribed state and local actions. *Total* preemptions supplant state and local with federal authority in a policy area. From the treatment of radioactive waste to the size and weight of trucks on interstate highways, federal rules have replaced state and local standards. Finally, *partial* preemptions permit "state regulation at the sufferance of Congress, which at any time may preempt totally the responsibility for a regulatory function" (p. 92). In the Water Quality Act of 1965, for example, Congress authorized the secretary of the interior (and later the administrator of the Environmental Protection Agency) to issue water quality standards that become effective within six months if states do not enact standards at least as tough.

Zimmerman's work is not the first on federal preemption. The U.S. Advisory Commission on Intergovernmental Relations, for example, published an important study called *Regulatory Federalism* (1984) that explored some of the basic issues. Zimmerman's book, however, is by far the most comprehensive on this critical question.

What most concerns Zimmerman is the erosion of the boundaries between the states and the federal government, an erosion promoted by Congress and abetted by the federal courts. At the core of Zimmerman's view of federalism is the concept of *imperium in imperio* (an empire within an empire), in which the states exercise certain powers relatively autonomously. With the rise of federal preemptions, especially since 1965, management has become more tangled, and accountability more elusive: "Federal preemption, total and partial, reduces citizen control of government as the decision-making forum is shifted from local governments and the states to Congress" (p. 13).

Accompanying Zimmerman's analysis is an impressive catalog of the forces underlying this trend. His chapter on informal preemptions, for example, contains thumbnail descriptions of 19 separate criticisms that have been raised against categorical grant-in-aid programs. Partial preemptions, he writes, have grown because of seven principal factors, including the failure of interstate cooperation and the failure of federal grants to provide enough inducements to shape state and local action. The formal preemptions that have arisen over the years number fourteen types. These catalogs and frameworks represent the book's strongest features. They are careful and exhaustive, laying out the groundwork and history of the issues.

At the same time, however, the book remains skeletal. Zimmerman suggests that only a dynamic model of federalism, based on mutual interdependence among the levels of the intergovernmental system, can account for the remarkable changes of the last generation; but he does not construct such a model. He conceives of the book as an exploratory study that he hopes will generate case studies to refine the broad outline he develops (pp. vii–viii). The reader might have wished Zimmerman's prodigious research and useful insights to lead past an initial exploration into both more case studies and a deeper examination of the issues. Furthermore, some important policy areas (especially the growing importance of Medicaid mandates) are not explored.

Nevertheless, long after general revenue sharing has become but a footnote in the federalism literature, mandates are likely to be an important issue. Zimmerman performs an invaluable service in drawing his readers' attention past the hot political battles surrounding federal grant spending to the quiet critical issues of federal mandates.

University of Wisconsin, Madison DONALD F. KETTL

COMPARATIVE POLITICS

Sadat and After: Struggles for Egypt's Political Soul. By Raymond William Baker. Cambridge: Harvard University Press, 1990. 365p. $35.00.

The Communist Movement in Egypt. By Tareq Y. Ismael and Rifa'at el-Sas'id. Syracuse: Syracuse University Press, 1990. 218p. $37.95.

Sadat and After attempts to answer the author's Egyptian students' question what kind of political community, "however fragmentary," they should try to fashion for themselves. Neither the official ideology nor development theory nor Marxist dependency theory were of assistance, because they reenforced these students' personal sense of powerlessness. According to the author, "political lives lived on the margins" are the resources they are utilizing to build a better tomorrow. The volume is about the margins of Egyptian politics, where alternative points of view offer competing views of Egypt's future. This pluralist alternative to the Egyptian state and to dependency theory (and, presumably, development theory) is said to be that of group theory accompanied by references to discourse and narrative self-descriptions and the potential power of all groups (p. 293). Instead, the reader is informed about such marginal groups and persons as the Arab Contractors (a construction firm) and Osman Ahmed Osman, who survived as an entrepreneur under Nasser, flourished in the economic liberalism of Sadat, and flourished (but less) under Mubarak. The Bar Association is described accurately as a redoubtable resistor of political authority under all regimes. Nasserism is presented as an enduring ideology, and Marxists are evaluated. The rugmaking and other artistic activities of the Wissa Wasef School are referred to as "creative empowerment," while the think-tanklike al-Ahram Center for Political and Strategic Studies is also described. Finally, the relative forthrightness of the important journal *Economic Ahram*, like the organization of the Muslim Brothers, is dealt with as a centrist politically important religious group.

The case studies are, in the final analysis, interesting and even empirically important. They provide glimpses of a more vital and diverse Egypt than do the stereotypical images of authoritarianism and the ritual of parliamentary democracy. However, although they are valuable vignettes, they are not empirically or theoretically very meaningful, in spite of the author's extensive efforts to make them so. (Two concluding chapters are devoted to this purpose.) First, not all the case studies are groups. For example, Osman Ahmed Osman is an individual entrepreneur, the Nasserists are a continuing important sentiment, and the Marxists represent an intellectual school of thought. Second, although the author speaks in his introduction of these groups in terms of their potential for power, in fact, among the eight, only the Bar Association and the Muslim Brothers have political importance, the others have either failed until now like the Marxists or are nonpolitical, like the Wassef School. Third, the author's determination to address groups on the margins of Egyptian politics and to theorize about them in such a restrictive way, keeps him from demonstrating theoretical and practical significance of his studies. This aside, however, the author has succeeded in informing us about the diversity of political voices in Egypt.

The Communist Movement in Egypt, 1920–1958 is essentially a chronological history of the myriad splinter organizations and personalities that figure in Egyptian communism. It is written by a well-known specialist on Middle East politics and a prominent Egyptian member of the party. Rifa'at el-Sas'id is presently the secretary general of the National Progressivist Union party, the major leftist opposition political party. The focus of the book is upon national organization, ideology, and personalities. It argues that whereas the first phase of the party's existence (1920–34) was one of external international initiatives, the second phase (1930–50) represented its Egyptianization. However, the authors fail to develop any interpretive themes as to the social background of its leaders or followers as the movement Egyptianized. Thus, the reader is presented with a succession of disjointed organizations that often appear to be parties of personalities, rather than institutionalized parties or parties with significant ideological differences. However, the movement was severely repressed both by Nasser and Sadat—in fact, el-Sas'id spent 14 years in prison.

The conclusion to the study is important because of its critical nature. It notes that communism was a Western doctrine adopted by Egyptians and that they have not been able to adapt it to the predominantly peasant-based economy. The author's comments that Marxism has become the intellectual conscience of the nation is also true. Some of Egypt's most respected intellectuals write regularly and critically about the problems of contemporary Egypt.

The book is useful as a chronicle of the movement but needs to be supplemented by Selma Botman's *Rise of Egyptian Communism* (1988) and Joel Beinin and Zachary Lockman's *Workers on the Nile* (1987). It contains a sizable appendix with translations of documents related to the party from 1915 to 1983.

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LOUIS J. CANTORI

The Politics of Social Solidarity: Class Bases of the European Welfare State, 1875–1975. By Peter Baldwin. New York: Cambridge University Press, 1990. 353p. $44.50.

The Welfare State in Capitalist Society: Policies of Retrenchment and Maintenance in Europe, North America, and Australia. By Ramesh Mishra. Buffalo: University of Toronto Press, 1990. 152p. $60.00 cloth, $19.95 paper.

Mishra and Baldwin offer contrasting thematic approaches to the study and analysis of the welfare state. Mishra's approach is to consider the degree of fragility of the welfare state under neoconservative attack. He observes that the neoconservative political movements in the United Kingdom and the United States led not to the dismantling of the welfare state but to the development of more class-stratified societies, in which the solidarity commitments of the welfare state to the poor and the needy were gradually eroded. On the other hand, he notes that commitments to maintaining the welfare state have generally been sustained in Sweden and Austria in spite of curtailment in the expansion of existing social programs. He discusses both the Canadian Progressive Conservative party's brief flirtation with, and retreat from, neoconservatism and the Australian Labour party's failure to move toward universalistic income maintenance programs as reflecting the unique political patterns of those two countries. Baldwin presents a historical critique of the theme of industrial worker "social solidarity" as a key explanatory variable in the development of European social welfare policies. He documents a variety of specific group interests, other than the interests of industrial labor, which have advanced social benefit programs and contributed to the development of the welfare state in diverse national settings.

While Mishra observes that neoconservatism, with its postulates of privatization and the maximization of free market activity, has not dismantled the welfare state and has been only partially successful in achieving its goals, what he does not fully explore is the fact that neoconservative successes in the United States have resulted in the erosion of the well-being of many of the poor. Nevertheless, the welfare state in the 1980s in Britain and the United States, as Mishra notes, "has not been dismantled by neo-conservatives. Much of the building stands intact though cracks are beginning to show, upkeep and maintenance have been minimal and the furnishings are beginning to look shabby" (p. xi). Here, he is on the mark as against analysts, Peter Ruggles and Michael O'Higgens, who minimized the ability of the New Right to advance its agendas in the United Kingdom and the United States (in *Stagnation and Renewal in Social Policy*, ed. Rein, Esping-Anderson, and Rainwater, 1987). The failure to develop a full employment policy in the United Kingdom plus the budgetary squeezing of the National Health Service have contributed to the development of a new, increasingly class-stratified society in the United Kingdom. Similar trends—albeit in a different context—can be observed in the United States.

Mishra notes that these social policies are tied to taxation policies that seek to minimize the impact of taxation on the wealthy and shift the impact of taxation to the middle and lower classes. What Mishra fails to notice is the success of New Right policies in the United States in paralyzing the development of needed social services in a period of rapid social change. While never fully examining the issue, Mishra does note in passing that, indeed, the Thatcher and Reagan policies created "a whole host of changes . . . significan[t] from the standpoint of social inequality and the social well-being of individuals" (p. 43).

Mishra also discusses balancing the goal of maintaining the social welfare state in Austria and Sweden with the goal of being economically productive and competitive internationally. For instance, he discusses the role of the tripartite Swedish Labour Market Board (government representatives and representatives of employers' and workers' organizations) in seeking to maintain full employment, universal social services, and basic minimum standards. Its emphasis on training, retraining, and relocation programs for workers has managed to deal with such dysfunctional tendencies as overmanning, labor rigidity, and low productivity.

Mishra observes that constraints exist on the neoconservative agenda of dismantling the welfare state, as well as on social democratic goals of advancing the welfare state. However, he noted that the neoconservatives in the United States and the United Kingdom have been successful in redistributing power and privilege upward, while social democracy (particularly in Sweden) has been able to defend social program commitments and full employment.

Peter Baldwin's book is an excellent comparative historical account that sustains a thesis that class interests in the development of social insurance programs are broader than the nature of working-class interests. Within this perspective, he undertakes to debunk the theory that labor solidarity is necessarily the key element to solidaristic social insurance policies. Among his examples are the role of the agricultural bourgeoisie in support of late nineteenth-century Swedish old-age social pensions and workmen's compensation insurance. Similarly, Denmark's noncontributory, tax-financed pensions appealed to Denmark's rural workforce of cottagers and smallholders.

In the post–World War II period, solidaristic trends to create more tax-financed social insurance–connected pension, survivorship, and health insurance funds resulted from a number of class interests. This included the importance of Liberal support (with a constituency of prosperous farmers) for universal pensions in 1953 in Denmark and of universal earnings-related benefits by the Danish Conservative party in 1964. Also in France, the groups that had too narrow an actuarial basis for social pensions and health insurance influenced the creation of insurance funds (regimes) organized by region, rather than by occupation, that included artisans and commercial and industrial firms. Similarly, in 1957, the Christian Democratic Union of the Federal Republic of Germany pushed the passage of a law that invited the self-employed (including lawyers) to join the general old-age pension fund retroactively and make retroactive payments. On the other hand, wageearner's representatives opposed flat-rate, universalist publicly financed social insurance measures, due to concerns of self-interest, not of solidarity.

Baldwin's key point is that narrow class interests, which would include working-class interests, often shape the context of social insurance legislation. Such interests are shaped by "sunken costs," that is, by previous legislation. Interest group pressure in such areas may vary dependently in relation to various economic and historical contexts. Claims for redistributive justice may be important, but the classes at the bottom vary from workers in Great Britain and Germany in the late nineteenth century to peasants, smallholders, and agricultural laborers in Sweden, Norway, and Denmark in the same period. Insightfully, the author observes that interests for and against solidaristic social policy reform are played out in terms of a triangular relationship between social insurance policy, society, and class. For example, the desire of the West German and French self-employed to eliminate the burden of being a narrow actuarial risk group and enter the wage-workers fund in the 1960s was initiated by both economic decline and the presence of socially separated insurance funds developed in the post–World War II period that had brought together for risk sharing all in the same occupational category. These events led those in "shallow risk pools" to support overall solidaristic policies—often over the opposition of the wageearners group.

Because these books are so different methodologically and in terms of their content, it is difficult to compare them. Nevertheless, both Mishra and Baldwin provide useful contributions to current public policy debates. Mishra's book is a significant antidote to public policy theorists who can see no impact on welfare state policies by neoconservative regimes in the United States and the United Kingdom. He also provides a lucid discussion of the political and economic circumstances that have led to a halt in the expansion of social welfare programs in Sweden and Austria, as well as a worthwhile discussion of the social policy quandaries of the Progressive Conservative party in Canada and the Labour party in Australia. Baldwin's book serves as a corrective to theorists who have viewed the evolution of European social welfare states too exclusively as driven by the policies of labor solidarity reflected in social democratic or labor movements.

University of Maryland HOWARD A. PALLEY

Identity, Competition, and Electoral Availability: The Stabilisation of European Electorates, 1885–1985. By Stefano Bartolini and Peter Mair. New York: Cambridge University Press, 1990. 363p. $59.50.

This volume is an exceptional achievement. Bartolini and Mair offer a theory of electoral stability in European democracies that covers most European democracies between 1885 and 1985. The volume's powerful argument, encyclopedic data base, conceptual richness, and methodological sophistication place it at the top of the reading list for those who would study European electorates.

The research flows from the scholarly agenda established by Stein Rokkan. Following Rokkan's advice, Bartolini and Mair "pin down numbers on the European variations" (p. 5), presenting numerous tables and graphs, as well as a detailed appendix filled with data on electoral volatility in Austria, Belgium, Denmark, Finland, France, Germany, Ireland, Italy, Netherlands, Norway, Sweden, Switzerland, and the United Kingdom. By examining aggregate data, they are able to cast a wide net, encompassing elections in countries for which there exist no reliable survey data and that occurred in decades before the advent of polling. The analysis divides a century of democratic competition into four phases: 1885–1917, 1918–44, 1945–65, and 1966–85. At its heart lies a set of related concepts and hypotheses: Electoral volatility (separated into bloc and within-bloc volatility) and cleavage (votes for a party or parties defined as representing a particular class) detail the dependent variable, electoral stability. A general framework offers change in electoral institutions, change in participation, party system format, spatial distance, cultural segmentation, and organizational density as explanatory factors; and a multivariate analysis demonstrates the importance of socioorganizational bonds (organizational density and cultural segmentation) and institutional incentives (institutional change and party system format) as persistent determinants of variation in the level of electoral stability. Variables associated with policy distance influence the dependent variable only in cases where socioorganizational bonds are weak. Bartolini and Mair maintain that European electorates are stable, that Lipset and Rokkan's freezing hypothesis stands, that social class has not declined as a primary basis of political identity, that the postwar era emerges as an extraordinarily stable period, and that recent changes do not mark realignments in electoral politics. The authors mount a spirited defense of Lipset and Rokkan and offer a path-breaking analysis of electoral stability in their own right.

Any judgment about the merits of the argument must address the authors' decisions about their concepts, measures, and exclusive use of aggregate data. Bartolini and Mair (1) treat aggregate levels of electoral volatility as if they were the same as the rates of individual vote switching and (2) test the freezing hypothesis by examining change across blocs of parties, rather than single parties. They provide intriguing, if not quite compelling, reasons for these two decisions. The first rests on finding both a relatively high correlation between vote switching as reported in surveys and as uncovered through the analysis of aggregate results and a striking argument detailing the relatively high probability that the correlation is not due to chance. At the same time, however, their argument ignores the tendency for all forms of data to report artificially high levels of vote stability. The second decision is justified by their reading of the relevant passage in Lipset and Rokkan's text; but their interpretation is not the only plausible understanding. The problem is that Lipset and Rokkan's claim is an empirical generalization, not a hypothesis embedded in a theory with deductively related hypotheses that could be tested. Similarly, the analysis of cleavage examines volatility by aggregating parties into blocs said to represent classes and offers a precise measure of organizational density (tapping how far persons who share a class category interact with each bloc) but then analyzes cultural cleavages with no measures of organizational density. Nowhere does the difference between their results and those obtained from surveys appear more clearly than in the analysis of the effects of social class on vote choice. Bartolini and Mair see strong and persistent effects, maintaining that class remains a powerful source of political identity. This stands in direct opposition to most political scientists, who see declining and weak, if any, relationship between class and vote. By staking out strong positions on these critical matters, Bartolini and Mair have helped to set the agenda for the next wave of research on European electorates.

Bartolini and Mair offer a model of how to do theoretically informed political analysis that remains close to particular events in particular countries. They address central theoretical and substantive questions, present powerful hypotheses, carefully construct variables and measures, and apply appropriate tests to their arguments. As a result, they teach us much about the electoral bases of European democracies.

Brown University ALAN S. ZUCKERMAN

Chinese Marxism in the Post-Mao Era. By Bill Brugger and David Kelly. Stanford: Stanford University Press, 1990. 223p. $32.50.

Philosophy and Politics in China: The Controversy over Dialectical Materialism in the 1930s. By Werner Meissner. Stanford: Stanford University Press, 1990. 230p. $37.50.

China and the Crisis of Marxism–Leninism. By Franz Michael, Carl Linden, Jan Prybyla, and Jurgen Domes. Boulder: Westview, 1990. 213p. $39.50 cloth, $18.95 paper.

These three books are all concerned with the nature and evolution of Marxist–Leninist ideology in its Chinese context, although their areas of focus and methodological approaches differ considerably. There is an underlying pessimism about the doctrine's future in all three books that perchance befits the times; but there is considerable variation in estimates of the intrinsic value of its contributions.

Werner Meissner, whose study focuses on the period of origin of a distinctively "Chinese" concept of dialectical materialism, is perhaps most witheringly pejorative in his assessment of its ideational content. The author's interest in the topic began with a reading of Mao Zedong, and he decided to trace the ideas set forth in "On Practice" and "On Contradiction" to their philosophical background in the intellectual controversies that raged over the "new philosophy" in Shanghai leftist circles in the 1930s. In his analysis of the works of Ai Suqi, Ye Qing, Chen Boda and others, he found them to be "strangely flat and lifeless" and "lack[ing] a genuine philosophical content, compared for example with the original works of Hegel and Marx": "Indeed, they are absolutely empty" (pp. v, 4, 5). He proceeds to dismiss their philosophical import, viewing them as "a self-contained system of analogous thought," a set of "symbols" or "totems" whereby the strategic and political conflicts being conducted inside party councils were expressed publicly, but in esoteric form. This is not immediately apparent upon reading one or two articles; only after reading the entire polemic are the various philosophical schools and their inner-party factional correlates discernible. Once this handful has been identified, everything falls into place, since the myriad lesser figures are found to repeat the themes and phrases of the more seminal thinkers with ritualistic precision. Indeed, Mao himself is found (in part 4) to have engaged in wholesale "plagiarism" (of Ai Suqi) in his work on dialectical materialism.

Meissner's approach is derivative of the "sociology of knowledge." Indeed, he acknowledges his debt to Mannheim. What is distinctive, however, is the scholarly thoroughness and rigor with which he applies it. Thus, the relationships between philosophy (i.e., Marxism) and science, perceptual and theoretical knowledge (concerned with differences and similarities, respectively), analysis and synthesis, induction and deduction, formal and dialectical logic, and dualism and eclecticism are all found to be an elaborate shorthand for debating the proper relationship between parties and armies in the second united front. After Xian—when the compromise was, in fact, negotiated—there are telltale shifts in philosophical positions. The author demonstrates the relationship between philosophical and political argumentation with considerable subtlety but finds it possible to conclude with a glossary that equates the two quite unambiguously.

China and the Crisis of Marxism–Leninism was first conceived by its four coauthors in the fall of 1986, when they presciently foresaw the dire turn of events that would culminate in suppression of China's democracy movement at Tiananmen in June 1989. Their book is essentially a collection of essays, focused for the most part on the post-Mao reform era but ranging quite freely over the entire forty-odd years of communist hegemony in China. Only Jurgen Domes focuses squarely on the ideological dimension, thereby providing a point of comparison with the other two volumes. Methodologically, his approach is not dissimilar to Meissner's; but he is less concerned with ideological controversy and its correspondence to inner-party factional and policy disputes than with the use of an authoritative general line to delineate the road to socialism that everyone must follow. During the pragmatic reform era, he finds the party doctrine to have become "somewhat blurred," with "only little development in the field of ideology since the summer of 1981" (p. 183). By, in effect, arresting further ideological development with his Procrustean four cardinal principles in 1979, Deng unwittingly precipitated a "crisis of ideology," manifested on the one hand by mass indifference to endlessly repeated official doctrines and on the other by spontaneously generated (and officially unwelcome) attempts to introduce ideological innovations by heterodox elements.

These spontaneous ideological contributions are of greatest interest to Brugger and Kelly, whose work constitutes the most sustained analysis yet of doctrinal developments in the post-Mao era. Their topical concerns thus coincide most closely with those of Meissner, focusing on the ideological controversy that flourishes before an authoritative line has been laid down. Yet methodologically, they are light–years apart; for while Meissner derisively dismisses the philosophical pretentions of his authors, Brugger and Kelly are interested in little else. "This book . . . is not a work of political science that seeks to trace networks of influence and power," the authors state: "It is about political, social, economic, and philosophical ideas" (p. 14). While the authors have a political subtext in that (like Domes) they hold Deng responsible for pretermission of the reform of Marxist dogma, this is the most purely "academic" analysis of Chinese Marxism that has yet appeared. The authors adumbrate Chinese discussions of the Hegelian notion of totality, functionalism and systems theory, structuralism à la Althusser, Jin Guantao's notions of the dialectics of nature, the objectivity of economic laws (such as the law of value), pragmatism (a favorite of Deng's), the implications of alienation, and Marxist humanism. These sometimes esoteric debates are related not to inner-party struggle but to parallel developments in Eastern European or Western Marxism (e.g., Lukàcs, the Frankfurt school, Popper). Perhaps this methodological shift reflects reality. If Meissner's Marxists were intellectual soldiers in a revolutionary war, contemporary Chinese theorists may wish to detach themselves from the political melee. In discussing Chinese Marxism's academic implications and growing integration with intellectual developments abroad, Brugger and Kelly have pointed to an important trend in its evolution.

From the perspective of political science, what seems to be needed is some sort of synthesis of these three approaches. A sociology of knowledge is still required, for the simple reason that much as some academic theorists might wish to detach themselves from politics, Marxist theory is still highly politicized—as is revealed not only in the continuing tendency of politicians to intervene in academic or literary debates but in repeated efforts by intellectuals to influence political tides. Yet to demonstrate the political utility of these debates surely does not necessarily imply a denial of their intellectual connotations. Lenin's contributions can also be shown to have had political utility, as, indeed, can many of Marx's. This is to be expected in any philosophical tradition built upon the quest not merely to rethink, but to change, the world. Thus, an academic analysis à la Brugger and Kelly is quite appropriate, though it should be leavened by somewhat more attention both to the political upshot (as in earlier writings by both authors) and to the relationship between ideological superstructure and political authority, which assumes pride of place in the approach of Domes and others.

University of California, Berkeley LOWELL DITTMER

East Asian Dynamism: Growth Order, and Security in the Pacific Region. By Steve Chan. Boulder: Westview, 1990. 134p. $35.00 cloth, $10.95 paper.

The Politics of Development: Perspectives on Twentieth-Century Asia. By Robert A. Scalapino. Cambridge: Harvard University Press, 1989. 137p. $19.95.

Scalapino's book stems from his lectures at Harvard in 1988, in honor of Edwin O. Reischauer. No doubt Mr. Scalapino was under no injunction other than to teach according to his best lights; and the result is graceful writing, informed and deliberate analysis, and plenty of food for thought. His book illustrates the bargain we all make as teachers. With the freedom to express ourselves as we see fit, we are obliged to explain what we know clearly, without artifice, and with respect for our audience.

Chan's book does not meet that test. It is often extremely patronizing; it overuses italics and boldface to highlight "special" words and concepts and the allegedly newfound dramatic conflicts faced by nations, which form the theme of the series; and it strains pretentiously to spell out the book's "architecture" and its "thematic elements."

Both books are concerned with roughly the same goal, namely, to describe, in a relatively brief and presumably an introductory way, the main patterns of East Asian development since World War II; and they often discuss the same issues. For both, the central question is what factors have led some East Asian and Pacific nations to achieve great and positive changes in their peoples' well-being while others have stagnated or even slipped back.

Chan, however, fights the evidence. While very familiar with the classical economic issues involved, he does not want to say that an economy based on individual incentives does the most for its people. In Asia (as elsewhere) the argument about the best path to development has revolved strongly around the conflict over export promotion versus import substitution policies. Scalapino knows where he stands, and tells us plainly. Import substitution has not worked and has led to other policies and structures (e.g., strong monetary controls) that have set nations like Argentina and Brazil behind where they were years ago.

On these issues, Chan equivocates, writing that "categorical assertions regarding the merits of either laissez-faire or command economies are not likely to be very informative" (p. 47). He also tells us (without any qualification) that "one of the hallmarks of the capitalist East Asian economies has been the political subjugation of their industrial workers" (p. 53). It is true enough for some, but what about Japan, or even Singapore? It is also hard to swallow his remark that the weakness of organized labor in East Asia has led its workers to fare in unemployment benefits, maternity leaves, minimum wage laws, occupational safety standards, and old-age pension funds much worse than their North American and even their Latin American counterparts (p. 53). It may be true of Costa Rica, but what about Mexico, Brazil, or Argentina?

His prejudices are evident also in connection with the much-maligned multinationals: "Fortunately for the capitalist East Asian [newly industrialized countries], the [multinational corporations] did not view them as attractive investment sites in the 1950s" (p. 55). In that period, most multinational corporations were active in the extractive industries; and neither South Korea, Taiwan, Hong Kong or Singapore are known for their mineral deposits. But why say "fortunately" unless to suggest that as a general matter, the involvement of multinational corporations is always negative for a nation's economy? But then, why does every developing nation today want to attract more, not less, multinational corporation investment?

At too many points, moreover, Chan is simply wrong on his facts. For example, he writes of the "inability of U.S. manufacturers to penetrate the East Asian markets" and asserts that "Japanese products sell much better than U.S. products in Taiwan, South Korea, and Southeast Asia" (pp. 52–53). Neither is true, as the quickest glance at actual trade figures over the past generation will demonstrate. American manufactured goods have held a rock-steady market share in some of these markets and have risen in others.

Scalapino writes, "I reject the view that imperialism was the controlling . . . factor in retarding China's development until 1937 . . . . The basic problems China faced . . . lay within China [and were] not governed by external factors" (p. 7). His message throughout is that internal factors and sound political leadership have been the crucial determinants for national improvement. He also continues his long effort to demonstrate the failures of Leninism and will infuriate many by saying that in Asia, two groups are potentially the "ultimate enemies" of democracy: the students and the media (p. 100).

But Scalapino shows us whence he arrives at these views. Untroubled by the trendiness that characterizes Chan, his is ultimately the far more satisfying effort. It is a sound introduction that will encourage beginners to explore further a mature synthesis with plenty of interpretation for the specialist to argue about. Chan, on the other hand, will only infuriate the specialist with his tendentiousness and misstatements, and his scholasticism will be off-putting to the novice.

University of New Hampshire BERNARD K. GORDON

The Political Economy of China's Special Economic Zones. By George T. Crane. Armonk: M. E. Sharpe, Inc., 1990. 204p. $42.50.

China's Foreign Trade Reforms: The Impact on Growth and Stability. By John C. Hsu. New York: Cambridge University Press, 1989. 221p. $49.50.

China's opening to the outside world is the most discussed aspect of China's economic reforms. China has become a major trading nation, receiving more direct foreign investment in the 1980s than any other Third World state. In contrast to the former socialist states of Europe, China's foreign trade and its economy have continuously expanded since the reforms began. In short, China has been integrated into the international economy, and is an increasingly important factor in world trade and capital flows.

These two books present different facets of China's involvement with the international economy in the 1980s. John Hsu's book is an economic examination of China's trade performance, particularly in the 1979 to 1985 period. He is particularly concerned with the issues of trade's contribution to economic growth and the effect of trade on inflationary pressures in China. In contrast with Hsu's macroeconomic and econometric analysis, George Crane offers an in-depth case study of the varied experiences of China's special economic zones, covering the period from 1979 to 1987. (Crane has updated his analysis in several recent publications for those who wish more recent details.)

Political scientists studying China will probably find the first half of Hsu's book more useful and accessible than the second part. However, it is in the second part where Hsu probably makes the greater contribution to scholarship. The first half of the book is a largely descriptive analysis of the role of foreign trade in China's economic development, both before and after the onset of the reforms, and a discussion of the changing commodity composition of China's trade both in general and in relation to its major trading partners, Hong Kong, Japan, and the United States. Hsu argues that there is a strong correlation between expanded foreign trade and periods of economic expansion in China. From this, he implies that trade causes domestic economic expansion in China. But because he does not break trade figures down by type of imports, his correlation and his inference may be overly broad. For example, grain imports will have little to do with economic expansion in China. Moreover, given that trade prior to 1978 was roughly about 5 percent of GNP at best, it would seem that Hsu has the tail wagging the dog with his argument that trade drives expansion.

Unfortunately, Hsu seems to have taken 1985 as his cut-off date out of convenience rather than as a date marking a fundamental turning or end point. At some point we all have to come to a point of closure; but by stopping in 1985, for a book published in late 1989, Hsu risks seeing his work outdated in short order.

The second half of Hsu's work is more interesting though much more difficult for political scientists studying China to follow. Here Hsu, drawing on the work of Richard Portes and others, tries to measure the effects of trade on consumer shortage in China. The work is heavily econometric, though the basic argument can be gleaned without too much trouble. Hsu concludes that trade since 1979 has had a less inflationary impact than trade prior to that date. The arguments about trade and consumer shortage are interesting, perhaps because political scientists studying China might not ordinarily think about such questions. Whether economists will be persuaded by Hsu's analysis of this issue is beyond my ability to assess.

Hsu's book is useful and there is much that political scientists can learn from it. It is already and increasingly dated however, and people familiar with Chinese trade will know much of the story already. Not surprisingly, it lacks almost any form of political analysis.

In contrast, George Crane's book is in many respects the inverse of Hsu's. Crane's work is an intensive case study of Chinese special economic zone (SEZ) policy based on political analysis, largely arguing that the dynamics of SEZ are explainable by internal dynamics of the Chinese state. Crane argues that "[p]olitical and policy instability are consequences of bureaucratic underdevelopment and ingrained personalism" (p. 163). By bureaucratic underdevelopment, Crane means that the bureaucracy lacks coherence, or that there is overlapping and confusing jurisdiction among the units of the Chinese bureaucracy. At the same time, he argues that the Chinese state is also overdeveloped to the extent of its extensive involvement in the Chinese case. Crane also makes the interesting observation that through the expansion of and publicity about SEZs, the conditions of their potential disappearance are being laid (p. 145). Other cities and regions all desire their own access to direct foreign investment and are lobbying the central government for their own special trading powers.

Crane's work is an insightful and useful study of the origins and developments of the SEZs that will be a constant reference source for people working on them. I think Crane's internal focus may be somewhat overexaggerated; after all, if foreign investors do not invest, or their investments fluctuate, it is not surprising that SEZs fail to live up to the (exaggerated) expectations placed upon them. Indeed, another way to read the story of the SEZs is that in bowing to investor concerns, the SEZ environment is increasingly attentive to foreign concerns. There may have been internal contention over the liberalization of SEZ terms, but the driving force remained the international environment.

Both Hsu and Crane would have us believe that the contradictions inherent in the Chinese reform process mean that reform will stall or fail. But we are now 5-7 years after the periods covered by these books. The Chinese have not made the kinds of reforms that both authors call for to maximize the benefits from China's opening to the outside world. Yet Chinese trade continues to grow and direct foreign investment continues to increase (despite the Tiananmen Massacre of 1989). All of this suggests that the dictates of economic theory or administrative rationality have little to tell us about the nature of the Chinese reform. Chinese leaders had to reform China given the structure of the system they created or inherited. They lacked the power and the inclination to undertake "shock treatment" to transform China's economy from socialism to capitalism. Given the comparative performance of China and the former socialist states of Europe, China's muddling through process of economic reform, with all its attendant contradictions, may have been the best model for other socialist or former socialist economies.

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Interest Groups and Political Change in Israel. By Marcia Drezon-Tepler. Albany: State University of New York Press, 1990. 308p. $49.50 cloth, $16.95 paper.

Stiff-necked People, Bottle-necked System: The Evolution and Roots of Israeli Public Protest, 1949–1986. By Sam N. Lehman-Wilzig. Bloomington: Indiana University Press, 1990. 213p. $25.00.

Struggles over the nature and purpose of Jewish political sovereignty dominated the early years of Zionist history. The founding fathers and mothers of the Zionist movement, who were Israel's political pioneers, engaged in extended debates over the extent to which a Jewish state ought to be "normal" and resemble other nation–states, as if by speech alone they could mold the characteristics of the Jewish state. Ironically, the debate itself seems to have had little pertinence for Israel's evolving society and polity. Implicit in Lehman-Wilzig's *Stiff-necked People, Bottle-necked System* and Drezon-Tepler's *Interest Groups and Political Change in Israel* is the notion that Israel has (1) become more normal as a democracy and (2) produced a more intensely Jewish political culture. In one way or another, Israeli political behavior provides the empirical, if disputed, terrain for observations on these two centripetal developments and specifically, why they have endured. Methodologically, the studies are quite different. Lehman-Wilzig's is a sophisticated statistical analysis. Drezon-Tepler's gathers data from interviews and the records of three important organizations with influence on policymaking: the Manufacturers' Association, the Ihud Kibbutz Federation, and Gush Emunim (the last committed to building Jewish settlements on the West Bank). Both studies are insightful and thoughtful, although the first may present a more useful angle of vision from which to evaluate Israel's polity.

In the literature on Israeli politics, scholars tend to focus on political parties and to discount the political influence of other organizations. With or without hegemony, contending parties have occupied ministries in coalition and often pursued highly divergent policies. Notwithstanding a high voter turnout, elections have not been decisive mechanisms for resolving policy conflicts or for determining priorities.

Aware that elections and other formal institutional mechanisms will not address issues of fundamental importance to citizens, a growing number have taken to demonstrations and protests. Public protests have come to play a significant part in communicating political demands to the established government. Electoral stalemate and an increasingly complex society have prevented Israel's elected rulers, whatever their political affiliation, from addressing many of the country's endemic problems. Without the high visibility of street protests and demonstrations, more and more Israelis of all classes and from all sectors—rich and poor, educated and uneducated, rural and urban—have come to believe that their needs, however legitimate, will be ignored. As political parties become more distant from their members, they no longer either provide accessible channels for communication or serve as agencies for resolving disputes. Protests, in part, redress the problems induced by proliferating bureaucracy.

Lehman-Wilzig explores the incidence of street demonstrations and extraparliamentary activities and generates a profile of the participants and their major demands. Attending also to their tactics, particularly the use of force and violence, he argues that public protests in Israel erupt because of a Jewish cultural disposition to be critical and a Jewish penchant for reform and because Israeli political institutions operate so unevenly.

Does the picture of Israeli political life so ably drawn hold any surprises? Only for those who assume that Israel's political development has been entirely exceptional. As Israel's economy has become increasingly industrialized and its population more diversified, traditional political institutions have been strained. In Israel's first two decades, a dominant political party achieved sufficient votes to broker a governing coalition and direct the most powerful ministries, including, among others, the office of prime minister, the ministries of foreign affairs and defense, and the treasury. Israeli political parties still provide an impressive array of services, answering many of the basic needs of Israeli citizens. Nevertheless, access to power and to the distribution of resources has always been limited.

Reflecting the willing recourse of Jews to criticism and reform, extraparliamentary associations and movements have continually cropped up to demand new and better solutions to old problems. In their marches and confrontations with authorities, however, protesters have mapped a political order considerably tangential to the one rooted in long-standing Jewish traditions or imagined by Zionist pioneers. While this political development was authentically Jewish, it, too, mimicked trends in other societies.

That interest groups operate in Israel is an indisputable assertion. Whether or not interest groups have decisive influence over policy is arguable. Subsuming Gush Emunim under the interest group category may strengthen Drezon-Tepler's claim that interest groups shape public policy; but the classification itself might invite considerable controversy. The claim that interest groups, rather than wars or territorial and demographic shifts, have transformed Israel's polity infuses Drezon-Tepler's analysis; but it, too, may be disputable. Many political developments in Israel, framed in the wake of international trends, seem way beyond the control of Israeli politicians. The Jewish state has become more entangled than ever in Jewish history in international politics. Israel's viability depends heavily on the balance of international forces.

The two books present contrasting comments on both the distribution of power in Israel and the process of political change. Drezon-Tepler posits that ongoing, multilayered contacts exist between government policymakers and the leadership of grassroots organizations, with these interactions shaping the contours of public policy. Lehman-Wilzig adopts a skeptical view of the pluralist model adopted by Drezon-Tepler and builds his argument around the notion that public protests signify the breakdown of communication and normal political procedures. Israelis take to the streets in order to be heard by the powerful.

Zionist history has imposed a weighty burden on Israelis and on Israeli society. For decades, aspects of this Zionist legacy have been subjected to criticism in the best works of Hebrew literature. Juxtaposing these two books suggests that political studies are beginning to join the humanities in probing the conventional wisdom from new perspectives. In the process, our knowledge and understanding of Israel's polity and society will be immeasurably enriched.

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Democracy and Markets: The Politics of Mixed Economies. By John R. Freeman. Ithaca: Cornell University Press, 1989. 311p. $39.95 cloth, $12.95 paper.

Conservative Capitalism in Britain and the United States. By Kenneth Hoover and Raymond Plant. New York: Routledge, 1989. 341p. $45.00 cloth, $16.95 paper.

With their election victories in 1979 and 1980, Thatcher and Reagan initiated campaigns (which were to last for the better part of a decade) against big government—the alleged source of the several economic ills that afflicted both nations in the 1970s. Both leaders won reelection by substantial margins. And for a time, at least, in the mid-1980s, as growth rates soared and inflation rates plummeted, conservative capitalism appeared to represent the necessary antidote to the stagflation that had plagued both countries in the 1970s.

Reagan and Thatcher are no longer in office; and as the United States and Britain have experienced once again in recent years low growth, accelerating inflation, rising unemployment, recession, and looming budget and trade deficits, the putative achievements of both administrations seem more ephemeral. That being the case, it is appropriate for scholars to compare and assess the enduring impact of the Thatcher and Reagan governments. One of the most detailed and explicitly comparative assessments to date is that provided by Kenneth Hoover and Raymond Plant in *Conservative Capitalism in Britain and the United States*.

Hoover and Plant argue that in their antipathy to big government, endorsement of markets, and efforts to shift resources from the public sector to the private sector, Thatcher and Reagan represented a more aggressively conservative form of capitalism than had existed in either government in the previous half-century. Arguing, correctly, that Thatcher and Reagan demonstrate the continuing importance of ideology in politics, Hoover and Plant begin their analysis by examining the core values and beliefs of conservative capitalism. The conservative critique enshrined monetarism, which implied reductions in spending and in budget deficits and increases in real interest rates; supply-side economics, which implied tax cuts to assist investment; deregulation; and privatization.

Why did Reagan and Thatcher become the symbols of the conservative critique of big government? Hoover and Plant suggest that while neither politician invented the critique (that was done, in bits and pieces, by academic economists, journalists, and other politicians well before either took over as party leader), both found the new conservatism not only ideologically congruent with what they already believed but very useful, as well, in their battles for the party leadership against more moderate and incumbent conservative leaders (e.g., Richard Nixon, Gerald Ford, and Edward Heath). Equally interesting, one surmises from Hoover and Plant that the new leaders' subsequent electoral successes in 1979 and 1980 may have had less to do with the specific tenets of this new conservative critique than with the persona of the leaders and the electorates' considerable distaste for the Democratic and Labour party incumbents and their policies.

Be that as it may, the two leaders governed for much of the next decade; and Hoover and Plant present an excellent and useful summary of the economic and social policies enacted by both administrations. If the discussion and appraisal of the implementation of those policies in Part 2 seems more detailed and complete for the British case, that may reflect the greater scope and implementation of the Thatcher program. Whether that was in fact the case (and if so, why) remains, unfortunately, something of a mystery, since the authors eschew a systematic comparison of the extent of programmatic and policy change in both governments, preferring simply to describe separately the policy of each. One wonders, for example, why Thatcher's government was considerably more successful than Reagan's in reducing the size of the budget deficit and (after the recession of 1980–81) the aggregate size of public spending relative to gross domestic product. Did this reflect Thatcher's greater commitment to the conservative critique? Did it reflect her greater capacity to impose her will upon her cabinet and government? Did it reflect the greater institutional coherence (some would say subservience) afforded by a three-term majority in a parliamentary system (in contrast to the House Democratic majority Reagan faced)? Or did it simply reflect the fiscal bonus generated by Thatcher's privatization campaign, a source of revenue not available to the Reagan administration?

The most provocative—and perhaps problematic—discussion in the Hoover and Plant volume appears in Part 3, where the authors attempt to provide the outlines of a response from the Left to conservative capitalism. The authors suggest that the Left (whose definition, particularly in the United States, remains imprecise) must accept the market, the need for economic incentives, private ownership, a certain "tolerable" degree of inequality in incomes, the centrality of production, and the constraints imposed on government by the economy. Then the Left can then develop a theory of needs, of which some (perhaps pertaining to health, education, welfare, and employment) could be provided by a "minimal" welfare state without adversely affecting the performance of the economy.

However well-meaning—and perhaps even laudable—these authors' suggestions may be, the discussion is, unfortunately, quite imprecise and general. We are told, for example, in regard to the United States, that voters would be offered "real choices," "realistic policies," and "credible programs." What those choices, policies, and programs would be is unclear, beyond the claim that they would aim to raise the incomes of the poor and provide jobs for all. How they would be funded (By cutting other programs? By raising taxes?) and how those objectives would be achieved (especially in the devastated inner core of urban America) remain unanswered questions. Likewise, we are not told how the Left would attempt to improve the efficiency, productivity, and rate of growth in the economy and, in particular, whether it would pursue distinctive policies to achieve those goals or simply mimic the conservative parties. Finally, we are not told in this otherwise excellent study whether the necessary reconstruction of the programs of the left parties is politically feasible or likely. Nor are we told why we should expect the voters of the two countries to be enamored enough of left parties with these new policies to provide the support necessary to recapture and hold national executive office.

As in Hoover and Plant's discussion of privatization in Britain, the role of public ownership figures prominently in John Freeman's *Democracy and Markets*. Indeed, *Democracy and Markets* is, above all, about the welfare implications of a mixed economy (i.e., one in which a significant portion of investment, production, and employment occurs in publicly owned enterprises) in an advanced capitalist democracy. In particular, Freeman seeks to elaborate how the presence or absence of significant public ownership (as well as of a corporatist mode of interest intermediation) affects both the performance of nations in regard to the level and rate of societal consumption and wealth of future generations ("*inter*generational equity") and the distribution of wealth between asset holders and wageearners ("*intra*generational equity"). How nations perform in these distinct aspects of collective welfare (more simply, using indicators from chap. 6, economic growth and distributional equality) depends critically, Freeman argues, upon the distinctive blends of a mixed, or private-enterprise, economy and a corporatist, or pluralist, mode of interest representation.

Despite the common concern with public ownership and private enterprise, the two books differ markedly in intellectual style. Whereas the Hoover and Plant volume is largely a description of the antecedents, driving ideologies, and practices of the Thatcher and Reagan governments, *Democracy and Markets* is driven by a more abstract and theoretical ambition to understand how and why capitalist democracies vary. Thus, while Freeman derives a typology of system types that yields a set of predictions about the welfare consequences of each type that can be compared with the actual outcomes of specific countries, he is not much interested in political parties, ideologies, or the policies of particular governments. Nor is he concerned with developing a full explanation of why the capitalist democracies vary in both the rate of economic growth over the long term and the degree of distributional equality. Instead, he is concerned with using the experiences of particular countries (Britain, Austria, Sweden, and Italy) to validate—and, if necessary, amend—his logico-deductive analysis of the characteristic blends of welfare outcomes produced in different types of political-economic system. And whereas Hoover and Plant conclude their study by framing the outlines of a programmatic response by the Left to conservative capitalism in Britain and the United States, Freeman concludes (also on a normative note, but more abstractly) by identifying what he believes to be the most desirable form of political-economic system in terms of achieving both intergenerational and intragenerational equity.

Freeman begins his analysis by identifying two dimensions along which the capitalist democracies can be arrayed. One involves the extent of public ownership of enterprises. The other involves the extent to which a corporatist mode of interest representation exists. Dividing each dichotomously thus yields a fourfold typology of political-economic system: corporatist with a mixed economy, corporatist with a predominantly private enterprise economy, pluralist and mixed, and pluralist with private enterprise.

Freeman then argues that each of these four system types produces a distinctive blend of welfare outcomes. The argument proceeds from two claims about corporatist and mixed systems: (1) corporatist systems achieve, all else being equal, higher rates of intergenerational equity (growth) and greater intragenerational equity (equality) than pluralist systems and (2) mixed systems achieve lower rates of intergenerational equity but higher levels of intragenerational equity than private enterprise systems. That being the case, corporatist-private enterprise systems should achieve higher rates of growth and greater distributional equality than pluralist-private enterprise systems. Corporatist-mixed systems should achieve lower rates of growth but higher levels of distributional equality than corporatist-private systems (and hence, in comparison to pluralist-private systems, roughly comparable levels of growth but considerably greater levels of equality). And pluralist-mixed systems should achieve lower rates of growth than either the pluralist-private enterprise system or the two types of corporatist system. But they should achieve somewhat higher levels of distributional equality than pluralist-private systems.

Having developed this complex set or arguments, Freeman moves to assess their validity. In this exercise, the author relies on four nations to represent the system types: Britain (pluralist-private enterprise), Sweden (corporatist-private enterprise), Austria (corporatist-mixed), and Italy (pluralist-mixed). After a brief cross-national analysis, Freeman discusses the performance of each of the four nations in detail and assesses the extent to which each nation's performance corresponds to the predictions. In the course of this discussion, he attempts to account for various anomalies that appear in the cross-national analysis or in the consideration of the four cases (e.g., the relatively high level of intergenerational equity in Italy but low levels in Sweden and Britain and the apparently low degree of distributional equity in Austria but higher-than-expected levels in Sweden and Britain). After a provocative discussion of the impact of openness on the welfare outcomes of the various system types, Freeman concludes the book with the analytic argument essentially intact. And shifting to a normative mode, he closes by arguing that based on the analytics, the corporatist-mixed type is the "most desirable form of democratic capitalism" because of its "superior welfare outcomes" (pp. 284, 286).

For all its analytic elegance, *Democracy and Markets* is not without shortcomings. Indeed, both those interested in the substantive issues of growth, stagnation, and distributional equity and those interested in, and informed about, European politics and economics will find this book frustrating and, in some respects, disappointing. For one thing, Freeman is engaged in what might be called marginal or partial theorizing. His ultimate ambition is not to explain why nations vary in terms of growth and equality but to ascertain whether different system types display different blends of those welfare outcomes. While the former task requires a full elaboration of all the factors that might account for variations in those outcomes and an assessment of their relative importance, the latter confines itself to only a few (in this case two) hypothesized sources of variation and requires only that discernible differences be observed among the system types. However elegant the supporting argument, such an approach is ultimately limited in scope and in its theoretical claims. Because of the inherent limits of this form of theorizing, readers who search *Democracy and Markets* for general explanations of why nations vary in rates of growth and distributional equity will find only a scattered assortment of ad hoc, country-specific factors brought forward to explain why apparent deviations from the theory-generated predictions do not weaken the theory. Thus, in regard to economic growth, various factors are enumerated to account for Britain's unusually low rate, including lack of private investment, the poor "entrepreneurial spirit," the overvalued currency, the exit option of capital, etc. Italy's unusually high rate of growth is attributed to the vitality of private enterprise. Sweden's low growth is explained by the deleterious effects of parliamentary and electoral politics, the concentration of Swedish capital, the decline of corporatism, and so on. A similar concoction of ad hoc, country-specific factors is mentioned to account for apparent deviations in the degree of distributional equity (although Freeman is less successful in accounting for these, perhaps because he largely ignores the distributional impact of taxation and expenditure policy). In other words, he specifies and evaluates neither the factors that systematically cause the rates of growth to vary across nations over the long term nor those which account for variations in degree of distributional inequality.

If the limited scope of the theorizing in *Democracy and Markets* will frustrate those concerned with substantive issues of growth and equality, the confrontation between theory and evidence will frustrate those who are knowledgeable about Europe. For example, the theory asserts that corporatist-private systems (Sweden) will have the highest rates of intergenerational equity and pluralist-mixed systems (Italy) will have the lowest rates. In fact, however, as Freeman's all-too-brief cross-national analysis suggests, the actual performances of the two were reversed. Indeed, over the period 1976–89, Sweden had an average growth rate of 1.8%, one of the *lowest* in the OECD and well below those of Britain and Austria (both 2.5%). And Italy had the *highest* rate of economic growth among Freeman's four cases, 3.2%! Similarly, while the theory claims that corporatist-mixed systems (Austria) will have the highest level of intragenerational equity and pluralist-private systems (Britain) the lowest, in fact (as Freeman acknowledges on pp. 173, 182), Austria's degree of distributional equity, in terms of income, is roughly comparable to that of Italy (one of the most inegalitarian systems in Europe) and considerably lower than that of Britain (or Sweden).

Such anomalies can, of course, be rationalized and explained in an ad hoc and unsystematic manner. But their existence may also be indicative of a fundamental mistake in theory and analysis. In particular, these several anomalies—involving, as they do, outcomes that are precisely the reverse of those predicted by the theory—suggest that Freeman may have misconceived the impact of corporatism, public ownership, or both on intergenerational and/or intragenerational equity. The magnitude of those anomalies suggests that the theory is in need of repair. Perhaps (some scholars might argue) the existence of a mixed economy affects neither the rate of growth nor degree of equality over the long term. Or perhaps it enhances (rather than diminishes) the former and diminishes (rather than enhances) the latter. And perhaps, (as the large literature on corporatism, only partially sampled by Freeman, might suggest) the impact of that form of interest representation on growth and equality is more complicated and varied than his theory implies. Perhaps corporatism enhances distributional equity only under certain conditions. And perhaps it enhances growth under certain conditions but diminishes it under others. Unfortunately, rather than considering these possibilities and the alterations in theory they imply, Freeman chooses instead, in the second half of the book, to preserve his theory despite the fact that it significantly mispredicts the performance of each of his cases on one or both aspects of collective welfare.

Whatever the real effects of a mixed economy and corporatism on economic growth and distributional equity, the incongruities between theory and reality seem so pronounced that Freeman must have either misspecified some or all of those effects or failed to identify other attributes essential to an explanation of why nations vary in those welfare outcomes. Thus, for all the analytic power deployed in *Democracy and Markets*, it seems to me that much remains unknown about why nations vary in level and degree of economic growth and distributional equality and how and why corporatism and public ownership affect those outcomes. And as a result, it seems at best premature—and in all likelihood simply wrong—to conclude, as Freeman does, that a corporatist-mixed system is the most desirable form of democratic capitalism.

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Hope for South Africa? By L. H. Gann and Peter Duignan. Stanford: Hoover Institution Press, 1991. 225p. $9.95.

A Democratic South Africa? Constitutional Engineering in a Divided Society. By Donald L. Horowitz. Berkeley: University of California Press, 1991. 293p. $24.95.

These two books, published in early 1991, are timely analyses of South Africa's democratic prospects—a long-debated question that has assumed special significance since President F. W. de Klerk's dramatic policy shifts announced in February 1990. The authors are guardedly optimistic, although they also agree that South Africa is a deeply divided—and deeply troubled—multiethnic country. They strongly disagree with Heribert Adam and Kogila A. Moodley's assessment in *South Africa without Apartheid* (1986) that South Africa is more a "common" than a "plural" society. In his first two superb chapters Horowitz presents abundant evidence documenting the depth of both interracial and interethnic conflict, exacerbated by what he calls the "metaconflict"—strong dissensus on what the conflict is about, especially the wide gap between the common society interpretation and Horowitz's own much more persuasive view that once white domination is eliminated, "intra-African differences will be particularly important" (p. 85). Horowitz also correctly points out that Adam and Moodley's benign common society diagnosis is contradicted by the "strong [consociational] medicine they prescribe" (p. 138).

A major difference between the two volumes is that Gann and Duignan devote a great deal of attention (almost half of their book) to an excellent systematic analysis of South Africa's economic and strategic importance in the international—regional and global—context (chaps. 6–7 and parts of chaps. 2–4). In chapters 8–9 they turn to political and constitutional options. They recommend a consociational, or power-sharing, system, based on proportional representation and coalition government, as the most promising method that can create a peaceful South Africa "albeit with great difficulty" (p. 167). In particular, they emphasize the problem of reconciling the insistence on unqualified majority rule and nationalization by the African National Congress (ANC) with the National party's attachment to minority group rights and private ownership. Their exaggeration of the South African Communist party's influence on the ANC adds to these worries.

Horowitz explicitly rejects power sharing, as well as other less frequently proposed solutions, like partition and the Zimbabwe model of white overrepresentation (chap. 4). He supports federalism, which can be a useful component of power-sharing systems; but he distrusts coalition building in proportional representation systems. Proportional representation does have the useful tendency to create a multiparty system in which no party has a majority of the parliamentary seats and in which multiparty coalitions have to be formed: "[But] the mere need to form a coalition will not produce compromise. The incentive to compromise, and not merely the incentive to coalesce, is the key to accommodation" (p. 171). Coalescence and compromise are, indeed, analytically distinct; and Horowitz cites plenty of examples of coalitions that have been unable to compromise and that, as a result, have fallen apart. But of course, there are also many contrary examples; and logically, the desire to coalesce implies a need to compromise. If parties are interested in gaining power (a basic assumption in political science), they will, in multiparty situations, want to enter and remain in coalition cabinets (a basic assumption of coalition theory); and then their only choice will be to reach compromises with their coalition partners.

While Horowitz underestimates the moderating effect of coalition building per se, he is right that additional incentives to compromise would be helpful. In addition to "seat pooling" (forming government coalitions), he therefore recommends "vote pooling"—electoral systems (especially the alternative vote) that encourage parties to appeal across ethnic boundaries. The alternative vote asks the voter to rank-order the candidates; if no candidate receives a majority of first preferences, second and subsequent preferences are redistributed until a majority emerges. For instance, if three candidates—A, B, and C—are supported by 45%, 40%, and 15% of the voters respectively, C is eliminated; and the ballots with C as first preference will be reallocated according to second preferences, making either A or B the winner. The great advantage, Horowitz argues, is that A and B have to bid for the second preferences of C's supporters in order to win, which will reward moderation. The problem is that precisely the same argument can be used—and is frequently used—in favor of plurality elections. In the same hypothetical example, A and B have to appeal to the voters who prefer C but do not want to waste their votes on C's hopeless candidacy or cannot vote for C because C wisely decides not to pursue a hopeless candidacy. Yet Horowitz rejects plurality, approvingly quoting Sir Arthur Lewis's well-known conclusion that "the surest way to kill the idea of democracy in a plural society is to adopt the Anglo–American electoral system of first-past-the-post" (p. 164).

The alternative vote resembles the majority runoff method even more closely, except that C is now eliminated in the first round, and A and B have to compete for the votes of C's supporters in the runoff. But it was exactly the majority runoff that was replaced with proportional representation in several ethnically heterogeneous European countries around the turn of the century because it raised ethnic tensions. Unfortunately, there is only one clear case of the use of the alternative vote in a plural society (Sri Lanka's presidential elections), where, Horowitz admits, it has not worked well (pp. 191–93). The dubious value of the alternative vote also weakens Horowitz's recommendation of presidentialism (largely based on the alternative vote's supposed moderating effect), which has to be weighed against the disadvantage of concentrating executive power in one person instead of sharing it among a broad coalition of leaders.

Since the completion of the two book manuscripts in the second half of 1990, the main South African parties have moved closer to a constitutional settlement and shown greater flexibility than most observers had expected. Horowitz worries about the ANC's "hostility to changes from first-past-the-post" (p. 268)—a concern shared by Gann and Duignan. But in the meantime, the main parties, including the ANC, have come out in favor of proportional representation (as well as parliamentary, instead of presidential, government). Such progress shows that there are grounds for even more optimism than these already reasonably sanguine books convey.

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Isolated States: A Comparative Analysis. By Deon Geldenhuys. New York: Cambridge University Press, 1990, 764p. $79.50.

It is a pleasure to report that Deon Geldenhuys has produced a truly impressive work on "isolated," or "pariah," states." It is both an exhaustive research effort and an important contribution to international relations theory and comparative foreign policy, combining historical analysis with an impressive array of statistics and rummaging into every conceivable corner of the subject.

The book's title is itself interesting. The author appears to be eschewing the catchy but value-laden and pejorative term *pariah states* in favor of the seemingly more neutral or objective *isolated states*. This also provides him a perhaps more favorable vehicle for extending his analysis back in time to search for historical precedents to the recent situations of Israel, South Africa, Taiwan, and Chile. But it is obvious that these four states are his central concern; and he is not inhibited about labeling them pariahs. In actuality then, pariah states becomes a contemporary subset of the broader and longer-term phenomenon of isolated states. The latter term is used to put the former term in historical perspective.

The book's strength is made apparent in two introductory conceptual chapters preceding the case studies: one laying out the framework of the problem with definitions, typologies, and categorizations, the other devoted to earlier (and some almost current) historical examples of isolated or quasi-isolated states.

Among the foci of the conceptual framework are isolation as voluntary or imposed by others and (tapping a vast theoretical literature) isolation as the counterpole of integration. Otherwise, the author lays out a panoply of variables used to measure isolation under the headings of political, economic, military, and sociocultural factors. In the political category are discussed the pariah image, international recognition, diplomatic relations (number of missions), membership in intergovernmental organizations and participation in conferences, involvement in international treaties, official visits to and from foreign countries, and levels of censure and support in intergovernmental organizations. Trade, investment, and aid statistics are comprised in the economic category; the military sphere encompasses military agreements, arms purchases, and military aid and cooperation either received or dispersed. The sociocultural factors include cultural agreements, travel, tourism, air and sea links, sport, art, entertainment, and academic and other exchanges. All of these variables are systematically examined and used to compare the cases of Israel, South Africa, Chile, and Taiwan.

The author's framework also entails an examination of the basic structural and functional components found in the process of isolation, suggesting how the empirical data might be fruitfully integrated. That involves, among other things, the target of isolation as directed primarily at the government or at interest groups or at the public at large (the nation itself); the means or instruments used (sanctions, boycotts, etc.); the origins of isolation and its rate of change (creeping, galloping, or sudden); the extent to which the very existence of the state (and not merely a regime) is at issue; the identification of the primary inspirers of isolation, whether a sole state (e.g., China) or a grouping (the Arab states, black Africa); the duration of isolation and the process of its exacerbation or mitigation; and the tendency for isolated states to constitute an identifiable political type, namely, what others might define as pro-Western, right-wing authoritarianism.

Otherwise, the author devotes some attention to secondary case studies as precedents and comparisons to his focus group, or "gang of four." There is extensive coverage of Bolshevik Russia, Weimar Germany, fascist Italy, Francoist Spain, Portugal before the 1974 revolution, the People's Republic of China, the former German Democratic Republic, Castro's Cuba, Rhodesia, South Korea, and Stroessner's Paraguay. On the basis of these cases, Geldenhuys says that the phenomenon of enforced isolation is by no means confined to the small group of pro-Western states popularly depicted as pariahs or outcasts. Indeed, communist, as well as right-wing, regimes have been represented. He stresses that the cases reveal degrees of isolation. And indeed, some of these states, such as the People's Republic of China or the Soviet Union, were restored to international "respectability" on their own terms—presumably not so much on putatively moral grounds as because they are not weak and vulnerable, like a Rhodesia. Still, I am unconvinced that the modern pariah state phenomenon is something other than historically unique.

That uniqueness, indeed, seems strongly underscored in the author's exhaustive comparative analysis of Israel, South Africa, Taiwan, and Chile. In his summary, the author carefully compares the four cases, showing that they each involve distinct profiles of isolation. Taiwan is the most isolated on the diplomatic dimension. It and Israel have the most-questioned national legitimacy. South Africa and Israel rank first and second in economic isolation; South Africa is the most isolated in the military and sociocultural spheres, with Israel close behind socioculturally. But each has a specific combined profile. Taiwan, for instance, is extremely isolated in the diplomatic sphere, much less so in the military area, slightly in the sociocultural realm, and hardly at all in economic terms. Chile, meanwhile, is shown to have been the least isolated. Indeed, it is now following South Korea out of "pariahtude," having become more integrated with the surrounding world even at the end of the Pinochet era.

The author ends with a good summary of the need for further research, particularly as regards the process of the "rehabilitation" of isolated states. That need has become more timely since Taiwan's security problems have mellowed, apartheid seems on the road to being dismantled, Pinochet is gone, and even the prospects for an Arab–Israeli settlement may have improved a bit.

The book's overall excellence allows for only a few quibbles. I have some questions about the extent to which the author seems to accept too easily the (cynical) grounds for ostracism of the isolated states by the so-called international community. The author, a Boer South African, seems, in short, to bend too far backward in identifying with his own nation's detractors—at times to the point of self-abasement apparently intended to play to an external audience. There is some confusing of moral issues with the realities of power, leverage, and ideological conflict. For instance, he says that "for many years, Taiwan has displayed the typical pariah feature of authoritarian government which has prompted adverse reaction before" (p. 93), without pointing out that perhaps as many as a hundred of the nations represented in the United Nation's international community have worse human rights records than Taiwan—many far worse.

For the graduate student or young scholar, Geldenhuys' work on isolated states can serve as an excellent positive example of how to do serious, empirical, comparative international relations research and still make it eminently readable.

Pennsylvania State University ROBERT E. HARKAVY

The State and Policy Outcomes in Latin America. By Lawrence S. Graham. New York: Praeger, 1990. 206p. $39.95.

Ascent to Bankruptcy: Financing Social Security in Latin America. By Carmelo Mesa-Lago. Pittsburgh: University of Pittsburgh Press, 1989. 290p. $49.95.

The State, Corporatist Politics, and Educational Policy Making in Mexico. By Daniel A. Morales-Gomez and Carlos Alberto Torres. New York: Praeger, 1990. 197p. $39.95.

Development policy can be conceptualized as a set of choices policymakers make in attempting to achieve economic growth, distributive justice, bureaucratic efficiency, and political participation. Each of these components is necessary for sustained development, and each affects the other three. For example, policies designed to promote economic growth affect the distribution of income, different parts of the bureaucracy, and which groups are permitted (and not permitted) to mobilize.

Lawrence S. Graham combines an overview of public bureaucracy in Latin America with a series of country (Brazil, Mexico, and Nicaragua) and regional profiles. Graham is particularly concerned with identifying strategies that can improve the effectiveness of development policies as they are formulated and implemented by national, regional, and local structures.

Carmelo Mesa-Lago's *Ascent to Bankruptcy* consists of the best available set of tables comparing social security statistics in all 20 Latin American countries and six case studies focusing on the evolution of social security policy in Costa Rica, Cuba, Chile, Mexico, Peru, and Uruguay. The title is based on the author's conclusion that the history of social security in Latin America is like a staircase leading to the abyss of financial collapse; that is, as the coverage of risks and population expands, so does the probability that the system will not be able to cover its costs.

Daniel A. Morales-Gomez and Carlos Alberto Torres analyze the conflicts and contradictions of educational policy planning in Mexico since the early 1970s. They examine the relationship among three elements: the politics of a corporatist state grafted onto the remains of a revolutionary political system, a revised model of associated-dependent development, and the process of policy formation in formal and nonformal education. The authors conclude, "The crisis faced by the Mexican State today is not primarily an economic crisis but a crisis of hegemony. It shows the failure of the dominant class to use the political, moral, and intellectual leadership that emerged with the Revolution to develop a new type of society" (p. 4).

Each of these books deals with the problem of bureaucracy in Latin America. By emphasizing the bureaucratic state-centered approach to development, most Latin American political systems have become dependent upon the rationality of their administrators in making public investment decisions, managing public enterprises, and providing a complex variety of social services. However, as Graham points out, the very success of many Latin American governments in having already created central bureaucracies now restricts further development: "Because their civil services have fulfilled essentially the roles of employer, guarantor of the status quo (insofar as the government in power is concerned), and defender of national autonomy, they have become an obstacle to further change over recent decades, especially when the imperatives of decentralization and autonomy—be they political or economic—are articulated" (p. xii).

The crucial point made by Graham is that despite all their attempts at political, economic, and administrative reforms over the past few decades, most Latin American governments have failed to enhance the quality of life significantly for large numbers of their citizens. In response to these failures, Graham claims that the most promising strategy is to explore the possibility of regional development as an effective compromise between centralized top–down and bottom–up (community-centered) reform efforts. He believes that mediating structures at the regional level (e.g., the state of Minas Gerais in Brazil) show the most promise for promoting democracy and mobilizing the necessary resources for local developmental needs.

Mesa-Lago is correctly concerned about administrative costs in implementing social security policy in Latin America. Whereas administrative expenditures in North America and Western Europe vary between 2% and 3% of total social security costs, in Latin America they are frequently above 10%. The author blames these high costs on "the persistence of a plethora of administrative agencies," contributing to inefficiency, inhibiting efforts at control and inspection, and facilitating evasion. He complains that because of political and/or union pressures, the social security agencies have often been used as employment agencies. Mesa-Lago also points out that in most of the Latin American countries, the rate of return on social security funds is only a fraction of the bank interest rates and that in many cases the real rate of return is negative. I wish the author had analyzed the role of corruption in the high administrative costs of social security.

The Morales-Gomez and Alberto Torres book deals with the political economy of education in Mexico. Education is often the most important and expensive distributive public policy, but it can also be viewed as an investment in human resources required for future economic growth. The 1917 Mexican Constitution committed the nation to the creation of a free, secular, and compulsory system of education, with little or no involvement from the private sector. By the early 1980s, the Mexican educational system had expanded to 24 million students and eight hundred thousand teachers. Yet despite this expansion, the Mexican educational system has been plagued by serious problems. In the early 1970s, for example, 30% of Mexican students were failing the first grade. By the mid-1980s, after great reform efforts, only about 50% of Mexican children were completing elementary school. The drop-out rates in rural areas continue to be high.

The authors argue that the success of the model of dependent economic development from the 1940s through the 1960s provided the Mexican political elite with the financial resources to support an expanding educational system. The political purpose of this policy was to legitimize a corporatist system that was retreating, in substance but not in rhetoric, from its revolutionary tradition. The authors claim that the fiscal and foreign debt crises of the 1970s and 1980s are forcing the political elite into new styles of technocratic thinking and experimentation in order to attain more legitimation from educational policy. In brief, they argue that the elite has tried to use educational reform as a substitute for more substantive distributive achievements. They predict that such efforts will no longer be successful.

These three books demonstrate the variety of developmental experiences in Latin America and suggest that no ironclad historical, cultural, or ideological determinism is at work; Alternatives are available. The dilemma is that so few developmental strategies can be created that are both politically feasible and economically effective.

University of Houston JOHN W. SLOAN

Authoritarian Power and State Formation in Ba'thist Syria: Army, Party, and Peasant. By Raymond A. Hinnebusch. Boulder: Westview, 1990. 350p. $54.50.

This book is about how the Syrian political system works. It is also an important effort to understand the political dynamics of a complex social order; and for this reason, the analysis has wide application to other authoritarian regimes. The author writes that the purpose of the book is "to examine the rise of the Ba'th Party, . . . to interpret the meaning of the authoritarian–populist state, and . . . to study the centrality of the linkages between elite and society." The book covers these topics successfully. In addition, it raises important questions about the tension between traditionalism and modernity and between political participation and political legitimacy. As such, this book is an important addition to the comparative politics literature.

Whereas this book faithfully attempts to explain the relationship between the Syrian state and society in depth, it falls short of discussing the informal processes that embody much of what makes the stuff of Syrian politics so interesting, namely, the role of social networks and the nature of antiregime political socialization. The formation of a counterelite in the form of political opposition such as the Muslim Brethren has deeper sociopolitical underpinnings than the author has portrayed. More probing in this area is necessary if we are fully to understand the role of "muted" political opposition to the Asad regime. After all, this type of political behavior deprives the regime of its most prized goal, political continuity—especially after Asad. Finally, the book attempts to discuss the political economy of Syria but falls short in delving into economic issues and problems that are responsible for Syria's continuing economic ills—ills that contribute to political alienation and disaffection. Aside from these observations, the book is well researched and thought-provoking. It is a useful tool to the serious student of Syrian and Middle East politics.

The book is divided into 10 chapters, the first and last of which are attempts to wed theory and history which I found to be thought-provoking and rich in explanatory and predictive capacity. Parts of the book cover the historical evolution of the Ba'th party, while parts focus on Asad's leadership style, the bureaucracy, and the military. The section on the military is sparse and, in some ways, weak, even though military institutions are the centerpiece of authoritarian regimes in general and Syria is no exception. While outlining the role of the Syrian military institutions in a historical context, the author fails to explain the nature of the relationship between the civilian population as a whole and the military. This conceptual flaw warrants further criticism.

Whereas the Ba'th party under Asad has been somewhat successful in increasing political integration of the periphery (outlying rural provinces and provincial towns) with the center (Sunni urban areas), this effort has not gone unnoticed by the Sunni citydwellers (as Hinnebusch correctly states). The efficacy of this integration remains limited, however, both because the traditional urban population is vehemently opposed to it and, more importantly, because of the persistent patrimonial nature of the Asad regime, which opposes multiple centers of power.

The Ba'th party has also failed fully to integrate the existing social forces (ethnic, tribal, and religious) into the armed forces. Hence, the armed forces, who are the true guardians of the "authoritarian" regime in Syria, are not only outside civilian control but also isolated (from within) and vulnerable to political temptations or corruption. Although the party has attempted to widen political participation, most players are exiled from key decisions or from Asad's inner circle of confidants, particularly in the foreign policy realm. Only through a process of cooptation and coercion (not voluntary obedience or a contractual relationship of political obligation and consent) has Asad been able to build a precarious power structure that pits the military against the civilian population, the military against itself, and the civilians against each other. Such a system is politically fragile and likely to collapse during a crisis—such as the sudden death of Asad.

Hence, Asad continues to play a political role with boundaries and responsibilities defined by neither civilian nor military popular support. The result, of course, has been a persistent crisis of legitimacy, one that continuously puts the Asad regime on the defensive.

U.S. State Department KAMAL BEYOGHLOW

State Failure: The Impotence of Politics in Industrial Society. By Martin Janicke. University Park: Pennsylvania State University Press, 1990. 171p. $29.95.

After the Rights Revolution: Reconceiving the Regulatory State. By Cass R. Sunstein. Cambridge: Harvard University Press, 1990. 284p. $25.00.

The politics of industrial countries in the 1980s retreated from relying on the government to regulate or intervene in the economy. Many industrial countries privatized the production of public goods and services, deregulated price controls and entry restrictions, and initiated a reversal of social regulation in health, safety, and the environment. The political impetus for these policies derived in part from a slowdown in economic productivity combined with growing inflation and a continual increase in the size of public debt and annual deficits.

Both of these books take a strong stand against this political trend toward economic laissez-faire and present arguments in favor of regulation and government intervention to help prevent and ameliorate the problems of modern industrial society. Advanced industrial society, in the view of these authors, has a list of problems, including an unequal distribution of wealth and income, discrimination, unemployment, inflation, damage to the environment, ill health and death from toxic chemicals, poor quality products, and industrial accidents. Neither of these authors, however, advocates big government, expensive subsidies, or the kind of regulation that causes inefficiency in markets and exceedingly high costs for minimal gains. They depict the difficulties and irrationalities of past policies and regulatory practices and offer new approaches to regulation and government spending and taxing that could become the future intellectual grist for the ideological mills of liberal and social democratic parties.

While both authors overlap in their concerns for reshaping the role of government in bringing about a more humane industrial society, the coverage, methodology, and approach of the two books differ substantially. Sunstein's book deals exclusively with the historical development, legal theory, and implementation of social, health, safety, and environmental regulation in the United States. He presents a cogent set of arguments for why regulation is needed and why it has failed. In the second part of the book he develops a set of interpretative principles for the courts in the regulatory state and shows how the use of these principles would improve regulatory decision making in a number of different applications.

His recurrent theme is that the common law principles derived from the constitutional period are woefully inadequate for the modern industrial state. For regulation to succeed, the courts must follow the reforms of the New Deal that introduced into the legal system "a sympathetic understanding of the functions of social and economic regulation" (p. 6). Under these reforms, citizens not only have political rights but also economic rights to health, safety, a job, and education. Sunstein seeks to reconcile and accommodate this twentieth century understanding of the state and public policy with the traditional legal concepts of constitutional government and private law. Sunstein's principles, in particular, are designed in priority order to 1) enhance political accountability and institutional deliberation, 2) protect constitutional rights for disadvantaged groups and protect against procedural unfairness and narrow interest group deals, and 3) to counteract statutory failure due to irrationality, unanticipated systemic effects, collective action problems, or other causes.

Janicke takes a much more sweeping and comparative approach to the question of regulatory and government failure. His thesis is a familiar one: successful and large producer groups, such as the construction, arms, finance, banking, and transportation industries, over time build more effective lobby relationships with the government administration than the average citizen is capable of building. Consequently, government panders to the needs of these groups through subsidies, tax abatements, weak regulation, and legal protections. This alliance between big business and big government creates a hierarchical and centralized power structure that stifles innovation, treats symptoms not causes, leads to economic stagnation and indecision, and drives out the courage to tackle the social, health and environmental needs of the society. He makes a useful comparative public policy contribution by applying these ideas in the areas of public health, the environment, transport, energy, and economic policies, and public budgeting and tax policies.

Since the basic inability of the society to pursue a humane industrial development is not a technical problem but a problem of power, according to Janicke, his solution proposes to break up the monopoly power of the industrial producers through strengthening countervailing powers of other groups and institutions in society. These actions to strengthen countervailing powers include reinvigorating cartel and antitrust policies, improving the legal position of citizens affected by industrial mishaps and costs, expanding political accountability to institutions in government that better represent more generalized interests not just producers, and decentralization to local units of government within a more effective central or federal coordination. Janicke proposes that citizens exploit economic and environmental crises as opportunities to build these countervailing powers in society.

Neither book's description and analysis of the causes and consequences of regulatory and state failure offer anything new. Sunstein's sections on the need for regulation and on regulatory failure, while covering no new ground, do serve as excellent and valuable reviews of this literature for courses in regulation, public policy, and public law. Sunstein also provides a highly valuable contribution to a multidisciplinary understanding of regulation by integrating his legal doctrine and philosophy into economic, rational choice, and political science literature on regulation and regulatory failure.

Janicke's explication of regulatory capture and collusive power between industry and the state neither offers a new explanation nor serves as a thorough and carefully presented review of the literature. Janicke leaves the reader confused on certain key points. While he maintains that producer groups dominate policy making, his own figures show that pensions and health constitute two of the greatest increases in state expenditures over the past decade. These areas serve a broad constituency of the elderly and the middle class not just, or even primarily, producer firms. Janicke also rails against the excessive military expenditures of the western industrialized countries, but in none of his figures are these expenditures shown to be very high relative to total public spending nor have they grown faster than other domestic spending. In addition, the book fails to explain how to exploit crises politically to overcome centralized power. This kind of inconsistency relates to a more basic problem: the book needs more carefully defined concepts and systematic empirical data on the areas under investigation.

The particular value of both of these books is not in their analysis of the problems but in their proposed solutions. Both books recommend ways for limiting the political strength of industry in regulation and government policy making. Sunstein's solution weaves an intricate carpet of ideas on legal interpretation that seeks to reconcile the constitution, private law, and the new social regulation. His ideas deserve attention from public law scholars, regulatory policy experts, practitioners, and political scientists. Janicke's section on solutions is both understated and underdeveloped. It is the place where he could make the greatest potential contribution to the literature but fails to do so beyond outlining what might be done. Yet his central idea that the solution to economic stagnation lies not in a technical approach but in new power relationships is critically important and deserves further elaboration and study by scholars interested in comparative public policy and political economy.

Michigan State University JACK H. KNOTT

Revolution: A Sociological Interpretation. By Michael S. Kimmel. Philadelphia: Temple University Press, 1990. 252p. $44.95 cloth, $15.95 paper.

Books that review theories of revolution, civil violence, and social movements continue to proliferate. Kimmel reviews over 30 theories in some detail and makes brief reference to at least a dozen more. James B. Rule's *Theories of Civil Violence* (1988) evaluated fewer theories but referred to a great many others for supporting evidence of one kind or another. Despite the different subject matter, about two-thirds of the theories in Kimmel's book are also covered, to one extent or another, in Rule's.

The sheer number of theories suggests that we know very little about revolution, rebellion, protest, social movements, civil or political violence, or other phenomena that are sometimes grouped under the same rubric and other times are not. If there are (as is probable) over 50 theories on the subject or subjects, there seems to be no way of knowing which, if any, is correct—or even has potential. Thus, books that evaluate what has been written about such phenomena are still useful. They help, if they are well done, in three ways: they provide students with overviews of a field that would be impossible to cover in a course of normal length; they group theories into categories and thereby provide ideas about common assumptions underlying each group; and they evaluate theories and may suggest useful ways to proceed in subsequent research.

The first type of help is provided by both the Rule and the Kimmel books. While some may quibble about interpretations of a theorist or which theorists are included or excluded, I think it is fair to say that both books provide enough background on prior theoretical work to give students a basic overview of the field. The second point is more difficult to evaluate. Rule uses a fairly standard categorization of the literature, with some interesting exceptions (such as tracing the intellectual roots of rational choice theory to Hobbes). Kimmel's categorization is more controversial. He divides theories of revolution into structural and nonstructural ones and argues for the merits of structural theories. By *structural theories* he means those that locate the revolution in an international context and analyze the effect of class conflict (defined in more or less Marxist terms) and the state on the potential for revolution. His justification for this view, however, is largely an argument by assertion that does not satisfy his own criteria for the evaluation of revolutions (pp. 153–56, cf. 3–12). In particular, he offers no empirical or theoretical support for his views.

Kimmel's criteria for evaluation of theories are much broader than Rule's. Rule largely assesses the merits of a theory by its falsifiability—a somewhat simplified criterion, given the multiple steps involved in deducing hypotheses, measuring key variables, evaluating and analyzing data, and so on (p. 13). Kimmel maintains that theories have to be internally consistent, explain various aspects of a revolution, and make sense of empirical cases. The use of theoretical criteria for evaluation is helpful. The addition of stronger criteria for empirical validation would add to the merits of the book. As it stands, a reader may disagree with Rule's application of the falsifiability criterion, but at least it is clear how it is applied. In Kimmel's case, the criteria for evaluation are often loosely applied and the reader is sometimes (although by no means most of the time) left with the feeling that the evaluation is done by assertion.

Given differences of subject matter and criteria of evaluation, it is surprising and somewhat reassuring to learn that Kimmel and Rule come to the same conclusion, namely, that Tilly's work forms the best basis for theoretical development. Kimmel uses Tilly as a synthesis of previous theories of revolution (pp. 206–216). His theory is "an admirable achievement" (p. 216). Rule views Tilly as having made "enormous contributions" to an understanding of civil violence (p. 199). Other theories do not fare as well, although both Rule and Kimmel appreciate the contributions of a wide variety of theorists. Nevertheless, both authors suggest that Tilly's work is probably the most useful starting point for further investigations in the broadly defined field of either revolutions or civil violence. Recent work in the field of social movements has emphasized the importance of looking at the structure of political opportunities, a perspective that also derives from Tilly's work. Thus, it appears that for the present at least, there is more consensus than might be imagined, given the multiplicity of theories. Various types of protests, including revolutions, are to be understood as fights among groups (usually including the state), who mobilize resources and gain allies to secure things that will benefit them. We are left with an assumption of "collective rationality" that may or may not prove useful in gaining more understanding of what has been an intractable problem.

Washington University BARBARA SALERT

Social Change in Modern France: Toward a Cultural Anthropology of the Fifth Republic. By Henri Mendras and Alistair Cole. Cambridge: Cambridge University Press, 1991. 254p. $44.50 cloth, $14.95 paper.

Democratizing France: The Political and Administrative History of Decentralization. By Vivien A. Schmidt. Cambridge: Cambridge University Press, 1991. 406p. $54.50.

Given the titles, these two books, *Democratizing France* and *Social Change in Modern France,* would seem to lend themselves to a single, integrated review. However, that is no easy task. Schmidt's title is an all-too-typical exaggeration. Her book is not really about democratization but decentralization, which, while an important topic, leaves us far short of democratization writ large. Mendras and Cole, on the other hand, understate their topic with their title, since their book is an attempt to cast as broad a net as possible over France since 1945. Both books have a lot to offer to anyone interested in France.

Schmidt includes everything a reader would ever want to know about decentralization, much of which will be new even to those of us who have done work on what the Mitterrand government called *le grande affaire du septennat.* Hers is an encyclopedic approach not only to the reforms of the 1980s but of similar attempts over the last two centuries. Schmidt does an especially good job with the legal and administrative aspects of decentralization.

Mendras and Cole cast a wider net and have written a very different book, the gist of which is better conveyed by the title of the French edition, *La seconde révolution française.* While one may question whether there really has been a second revolution, the authors do a first-rate job of charting the sweeping social and economic changes of the last half-century as well as their equally sweeping political impact, including the decline of ideological divisions, political stability, and a deeply rooted consensus in support of the Fifth Republic. While there may be little that a French specialist has not seen before, the book is a treasure trove of insights into the way France has changed over the last half-century, ranging from statistical analyses to the comparison between nineteenth-century bourgeois dinners and contemporary barbecues to illustrate just how much things have changed.

The difference between the two books can best be seen in the way the authors deal with the one subject they treat in common, local politics. Schmidt treats it far more fully but does so without giving much of any consideration to the other aspects in French politics that help shape it or raising any of the broader issues that would be of interest to comparativists for whom decentralization in France is not a top priority. Those connections leap from almost every page of the less detailed but more provocative volume by Mendras and Cole.

Both books also have their problems. Schmidt really provides more than anyone who is not personally conducting research on decentralization in France needs or wants to know. Moreover, her failure to address any of the larger issues will limit the impact of her brilliantly researched book, becuase of failing to address comparative scholars in general.

On the other hand, Mendras and Cole probably cast their net too widely. In part because Mendras, the primary author, is a sociologist, much of what they cover will not prove to be all that interesting to many political scientists. Moreover, many of their most sweeping generalizations could use a lot more empirical support, while the statistical material they do present tends to be drawn from sources that are too disparate to convince a skeptical reader, especially in their analysis of the declining importance of class.

Both books are first-rate examples of what modern scholars can do, albeit examples drawn from very different scholarly traditions. *Democratizing France* will be an invaluable resource to the handful of us who care about decentralization policy in France since 1789. *Social Change in Modern France* is nowhere near as comprehensive or encyclopedic. But in Mendras and Cole's broad treatment and in their tables (as well as their metaphors) there is some information. More importantly, there are intellectual challenges to anyone with even a passing interest in either France or sociopolitical change in advanced industrial societies. Both succeed in meeting the goals the authors set for themselves. Students of French and European politics should give each serious attention.

Colby College CHARLES HAUSS

China Misperceived: American Illusions and Chinese Reality. By Steven W. Mosher. New York: Basic Books, 1990. 260p. $19.95.

Mosher examines American perceptions and misperceptions of contemporary China. While following in the tradition of Harold Isaacs's classic *Scratches on Our Minds* (1958), he attempts to go further and "distinguish between truth and fiction." Unlike Isaacs, he eschews the visual media, focusing, instead, on the writings of scholars, journalists, and tourists and offering a partial and caustic view of the "China watcher" community, mainly since 1949. At the same time, Mosher recapitulates the national and international context that conditioned the familiar swings in American "love–hate" attitudes toward China.

Mosher briefly recapitulates the traditional conflicting images of China as laid out by Isaacs and the predominantly admiring views of American correspondents there prior to, and during, World War II. He usefully summarizes the fascinating retrospective conference of many of these journalists published in MacKinnon and Friesen's edited volume, *China Reporting* (1987). An excellent chapter on the "age of hostility" links the McCarthy hysteria over "who lost China" with Beijing's alliance with Moscow and entrance into the Korean War to explain the next two decades of largely negative writing. The sudden swing back to the admiring themes of World War II after the dramatic Kissinger–Nixon visits to Beijing in 1971–72 muffled the mayhem of Cultural Revolution violence, thanks to "an informal but effective coalition of admiring China-watchers and conservative realpolitikers" (p. 33). Then, post-Mao revelations with Deng Xiaoping's ascendancy in 1978 started a reverse mood of disillusionment, followed by a decade of "benevolence" in the belief that capitalism was replacing communism. The Beijing massacre of 4 June 1989 abruptly closed this period, introducing what Mosher sees as the end to American illusions with the blinding truth of Chinese reality.

Mosher offers a searing indictment of American journalists and scholars for their favorable but distorted depiction of Chinese reality; but he also has little praise and virtually no specific citations for those who had it right, unless they are from the Right or have subsequently acknowledged their earlier error. The late John K. Fairbank becomes a prototype of the academic who tailors analysis of China to fit a pattern suitable for influencing a particular audience in either country, depending on the writer's motive at the time. The case against Fairbank as a dispassionate historian and observer is closely argued, including a frank acknowledgment of partiality by the acknowledged dean of American sinology. To his credit, Mosher avoids invidious assessment of intent except when explicitly admitted (as by the wartime correspondents post hoc and Fairbank himself).

However, Mosher's selection of writings implicitly portrays the bulk of American sinologists as fools or knaves. He makes a good case with the quotes he offers but is not comprehensive either in his examination of a single author or in his coverage of the field. The absence of a bibliography underscores this selectivity. Casting a wider net would have captured an impressive number of rigorously objective and accurately analytical American scholars whose carefully balanced work has stood the test of time, despite the obvious obstacles for empirical research.

University of Arizona ALLEN S. WHITING

Education Reform in Japan: A Case of Immobilist Politics. By Leonard Schoppa. New York: Routledge, 1991. 319p. $59.95.

The central question of this book is, Why has the Japanese government failed in its efforts to reform education? The question is answered theoretically, by first establishing a modified Pempel–Campbell model of conflict resolution pattern in Japanese policymaking. Within this framework, the author hypothesizes that

> (1) low-conflict disputes—some combination of high divisibility, wide scope and low affect—tend to result in incremental change or . . . "contrived consensus" type decision-making; (2) high-conflict disputes involving *outside forces* result in camp conflict and either (a) forced resolution by the government or (b) opposition success in convincing some conservatives to back down, [resulting in inaction]; (3) high-conflict disputes involving primarily *conservative camp actors* result in conflict avoidance and little change." (p. 20)

The essence of the policymaking process in Japan is to get "consensus *within* the conservative camp while not requiring the approval of non-conservative forces" (ibid.).

The book's organization is tied closely to the Pempel–Campbell model, for which the author provides the theoretical background in his opening chapter. He goes on to identify the major conflicts in the current battle over education reform and then treats divisions within the conservative camp in the ruling Liberal Democratic party (LDP), the bureaucracy, and special interest groups. After discussing the role of the progressive opposition, particularly the Japan Teachers Union, he tests his model by examining the education reform movement in the 1970s and in the Nakasone era.

The author concludes that the education issue has been characterized by several patterns of conflict and that the resultant policy decisions could be predicted by the modified model. For example, the educational expansion program of the 1970s represents Pempel-Campbell type 1, the Shoninsha Kenshu program under Nakasone is typical of type 2a, and the proposal for unifying nursery school and kindergartens is type 3. The author argues that the long-standing dominance of the LDP has led to an internal diversity that has obstructed political change. The paralysis in the policymaking process usually occurs when rifts develop between bureaucrats, party leaders, and politicians specializing in education; but it afflicts other areas of Japanese life beside education, as well.

The author's conceptual framework is original and is presented in a logical, straightforward, and remarkably lucid manner. The data with which he supports his views in each chapter are informative, well balanced, and recent; and his sources of information are useful and usually reliable. Figures and tables, a glossary of Japanese terms, and references are not only accurate but conveniently placed.

The author's model of conflict resolution patterns may be challenged by the *household* (*ie* or *uchi*) model advocated by many Japanese scholars, including Chie Nakane, Seisaburo Sato, and Eiichi Kato. It is impossible fully to explain the household concept here, but clearly it has a vastly different conceptual foundation. To the Japanese, *ie* is a corporate residential group formed on the basis of an established frame of reference—a managerial group, whether of a family, a business corporation, or a community. According to Nakane, the household concept, which puts great emphasis on relationships, trust, personality, and group unity, actually restrains the strong-willed individual. The leader, who usually exerts his power indirectly through his immediate subordinates, must have their consent to adopt a new policy or reform an old one. The process of consensus building (*nemawashi*), in Kato's words, is perhaps more important than formal legislative debate. Advocates of the household model may feel that Schoppa has put too much emphasis on issues and conflict, and too little on personalities and consensus building.

American scholars, meanwhile, may object to the lack of clarity of such concepts as *high-conflict dispute* and *conservative camp*. Those familiar with Herbert Simon's criticism of Luther Gulick's four principles of administrative organization (purpose, process, clientele, and location) on the ground of imprecise definitions of terminology may find that the same criticism applies here.

Nevertheless, the merits far outweigh the faults, and students of Japanese affairs are fortunate to have it. One of its strongest points is that the author has discovered an Achilles' heel in Japanese politics—the Japanese cannot make national policy decisions when there are strong division between bureaucrats, LDP leaders, and legislative specialists. This provocative finding will challenge the belief—increasingly widely held in the West, as Japan's economic power has grown—that the Japanese invariably make the right decision at the right time. Whether his other findings will be borne out must still be seen.

Gannon University PAUL S. KIM

Liberty and Community: Canadian Federalism and the Failure of the Constitution. By Robert C. Vipond. Albany: State University of New York Press, 1991. 349p. $14.95 paper.

There has been a spate of books on Canadian federalism in recent years, as there has been on American federalism. Both systems have felt a variety of cross currents as the role and the power of the central government shift and the subnational governments seek appropriate responses. Most of the recent books on Canadian federalism have focused on the impact of the Constitution Act of 1982 and the reforms it introduced into intergovernmental relations in Canada. None of the recent analyses have found a solution to the long-maintained interlevel struggle. Few of them are based on historical examinations of Canadian political thinking.

What Vipond argues in this carefully reasoned book is that the "constitutional debate has . . . center[ed] on two 'competing conceptions' . . . of what the country is or ought to be; conceptions which [have] helped to polarize political debate"—namely, the *liberty* and *community* of the book's title, meaning the liberty of individuals versus the several communities' interest in "collective choice" (p. 2). In other words, it pits the central government as defender of individual rights against the provinces as guardians and proponents of regional community differences and desires. But the evolution of Canadian federalism, Vipond argues, need not be based on that dichotomy; and therein lies the original thesis of his work, which is basically a historical review of the early federal debate in Canada.

Although Vipond recognizes that the tension between liberty and community in Quebec has received the most attention by scholars and practitioners alike, he chooses to study in depth the Provincial Rights movement in Ontario from confederation in 1867 until roughly 1990. He does so because in that movement he finds that the proponents of provincial rights rejected the incompatibility of liberty and community and argued, instead, that the provinces—the separate communities of Canada—were also capable of protecting individual liberties. His arguments are convincing.

His approach is clearly laid before the reader in his introductory chapter. As he proceeds, he seeks to lay the basis for a shift in the future thinking about Canadian federalism from the either/or stalemate of the last quarter-century, which has caused the Canadian polity to lurch "from one constitutional crisis to another" (p. 1), to a recognition that both concepts may exist together within the broad historic contours of the Canadian view of liberalism. He does not dot every *i* and cross every *t* in the debate over provincial autonomy in Ontario during the chosen time period but concentrates on what he considers to be "pivotal episodes" in that long debate (p. 13). In successive chapters, he looks at the concept of sovereignty in Canadian thought before 1867; the British North America Act's provision for a centrally appointed lieutenant governor of each province and consequent impact (or lack thereof) on provincial autonomy and self-government; the lessons taught to Canadians by the broader concept of imperial home rule as clues to the federal relationship in Canada; and the deep faith of debaters on both sides in the rule of law as the fundamental basis of both liberty and community.

In his well-argued conclusion, Vipond demonstrates that in the Ontario debates proponents of provincial rights (of the provinces as independent political communities) based their defense "on a deep commitment to liberal individualism" and an equally deep commitment to the view that the people were "the source of, and the ultimate check on, governmental authority," wherever it might be exercised! "The ideology of provincial autonomy, in short, was both considerably more ambiguous and considerably more subtle" than it came to be seen in later years (pp. 194–95). Vipond begs his readers "to see beyond, or see through, the 'competing visions' that animate, but also divide most constitutional discussions" in Canada today and to understand that underlying both concepts is the "common commitment to liberalism" of all Canadians (p. 196). In his view, the principles of liberalism are sufficient today (as they have always been) to allow a resolution of the Canadian constitutional crisis. That is what he sees as the challenge to Canadian federalism and perhaps its ultimate solution.

Some 45 pages of chapter notes give evidence of Vipond's careful and thorough research and understanding of the complexities of his theme; the index is unusually well constructed. It is a most thought-provoking and constructive book.

Duke University RICHARD H. LEACH

The Child and the State in India: Child Labor and Education Policy in Comparative Perspective. By Myron Weiner. Princeton: Princeton University Press, 1991. 213p. $45.00 cloth. $14.95 paper.

The core proposition of Myron Weiner's study of child labor is that India's low per capita income and economic situation is less relevant as an explanation for the high numbers of children in the labor force than the belief systems of the state bureaucracy (p. 5). "It is the absence of strong support for governmental intervention within the state apparatus itself and the absence of a political coalition outside the state apparatus pressing for governmental intervention that explains Indian policy (p. 15)." Weiner's confirmation of the proposition leads him to the "pessimistic conclusion that barring a conceptual change in the thinking of those who make and implement policy and a new direction in policy by the Indian government, the number of children in the labor force will not significantly decline . . . (p. 207)."

The discovery that the bureaucrats show little interest tells us not that the situation is hopeless, but rather where campaigns for change need to be undertaken. The other countries Weiner studied also went through phases in which their bureaucrats were as disinterested. Many countries allocate insufficient funds to education, because they are poor or they hold education in low priority or both. Many countries spend a good deal on education but favor the middle and upper classes. Weiner provides a useful history of attempts to implement compulsory education in India and, for comparison, in advanced industrial countries. He might have also discussed the complementary idea of a right to education, under which governments could be called to account if adequate educational services were not provided. As a result of a decade of negotiations, a new Convention on the Rights of the Child came into force in September of 1990. The convention devotes several articles to labor and education. Remarkably Weiner gives no attention to the idea of children's rights. He discusses the rights of parents and the rights and obligations of the state but does not explore the idea of children's rights. India has not signed the convention, but the developing consciousness of childrens rights worldwide will nevertheless have an impact on India.

Weiner repeatedly recognizes that Indian schools are not good at building human capital, and thus are not attractive to the poor: ". . . the Indian educational system is defective because it does not prepare village children and the urban poor for living and working in their own environment (p. 187)." Education generally is not compulsory and most schools are not good enough to attract voluntary attendance. The solution does not lie in simply passing more laws limiting child labor or requiring school attendance, both of which reduce the families options, but rather in improving the quality of the schooling opportunities available to children. In the 1930s Gandhi, in his journal Harijan, argued that:

> As a nation we are so backward in education that we cannot hope to fulfill our obligations in this respect in a given time if this programme is to depend on money. I have, therefore, made bold, even at the risk of losing reputation for constructive policy, to suggest that education should be self-supporting . . . I would, therefore, begin the child's education by teaching it a useful handicraft, enabling it to produce from the moment it begins its training. Thus, every school can be made self-supporting, condition being that the State takes over the manufacture of these schools (p. 105)."

Gandhi's scheme for paying for education by marketing items produced in the schools was not implemented, possibly because of resistance from private manufacturers of similar items.

The fundamental dilemma of child labor is that parents feel they cannot forgo the current earnings of the children in order to invest in building their "human capital" and thus enhance family income at a later time. A well designed loan is precisely the instrument needed to transform the anticipated future earnings into present benefits, sufficient to pay tuition and to compensate for forgone short-term income. Perhaps private vocational schools could be created, with curricula designed to build the skills that would enhance long-term earning capacity in the local setting.

Through the Grameen bank experiment in Bangladesh, micro-enterprise loans through the United States Agency for International Development, the Accion International organization, and many other programs, it is now being recognized that novel loan arrangements have remarkable potential as instruments of development. India, already familiar with the institution of bonded labor, should have no difficulty with the concept of long-term loans designed to enhance the individual's earning capacity.

India's massive child labor problem cannot be addressed simply by passing new laws. There is a serious need for strategic planning and for creative institutional design. Weiner's study advances our understanding of the problem, but there is much more work to be done to move toward meaningful remedies.

University of Hawaii GEORGE KENT

Transforming Settler States: Communal Conflict and Internal Security in Northern Ireland and Zimbabwe. By Ronald Weitzer. Berkeley: University of California Press, 1990. 292p. $29.95.

How can ethnically divided societies become genuine democracies after decades of conflict? Weitzer marries a Weberian conception of the state to the new institutional literature and then compares the evolution of the security services in Rhodesia/Zimbabwe and Northern Ireland in search of an answer to this question. Weitzer argues that a necessary-but-not-sufficient condition for a successful transition is that their internal security bureaucracies can be professionalized and rendered "ethnically" neutral to a significant degree. Weitzer sees the security apparatus as the core of the settler state, which by its very nature rests on physical coercion of an indigenous population. Only a security apparatus subordinated both to law and to the civilian parts of the state can generate enough legitimacy among the population (particularly formerly oppressed ethnic groups) to allow consent to prevail over coercion as the major determinant of people's behavior. To the extent that the new security services do not approach this Weberian ideal type of a neutral, rule-governed bureaucracy, they either can be turned against some new population or will not be able to generate legitimacy and consent from the population. Weitzer's argument thus addresses issues familiar to those studying democratization in Latin America and Eastern Europe, as well as more traditional students of state building in decolonizing areas.

In both of Weitzer's cases, genuine democracy remains a distant goal. In Zimbabwe, the Zimbabwe African National Union (ZANU) under Robert Mugabe simply appropriated the old security apparatus wholesale, replaced whites with Shona tribesmen loyal to ZANU, and then used the security system to destroy the rival Zimbabwe African People's Union (ZAPU), which was associated with the minority Ndebele tribe. Mugabe's party merely changed the ethnic composition of an ethnically dominated state. In Northern Ireland, the collapse of the (Protestant) settler state's control over Protestant militants and hegemony over the minority Catholic community led the British state to usurp the Protestant state's security functions. In this situation, both communities ended up distrusting the security services. This made any transition to a locally controlled state security service impossible, even though the security apparatus was partially liberalized and bureaucratized.

The strength of this book lies in its discussion of the legal and institutional trajectories of the security apparatus in these two countries. Weitzer nicely situates this discussion in the origins of the collapse of the two settler states, showing how the security services won an increasing but counterproductive autonomy from civilian control. But this discussion reveals the implausibility of successful and peaceful liberalization of the security services.

First, Weitzer argues that the security systems' personnel themselves increase ethnic discord by eliminating legitimate channels for dissent and militarizing the state's responses to extralegal ones. He argues that a dialectic of repression and resistance soon transforms the relationship between the security services and the state. As resistance increases, the civilian (but settler-dominated) parts of the state grant extraconstitutional powers to the security services and waive any efforts at oversight. Precisely this phenomenon suggests the impossibility of reforming the security apparatus. As these services generate newer and harsher responses to indigenous resistance groups, they become increasingly *debureaucratized*. Standard operating procedure gives way to a desperate search for spectacular one-shot "solutions" engineered by highly trained and essentially lawless commando forces like the Selous Scouts and the Special Air Service. Indeed, as the Birmingham Six trial showed, this lawlessness can metastasize back into the metropolitan state itself.

Second, in settler states with a low ratio of settlers to indigenes, the settler population in its entirety often *is* the state and (particularly through obligations to serve in the reserves) the military. This was certainly true in Rhodesia (5% European), and almost true in South Africa, where Afrikaners are primarily employees of the state and state-owned corporations. "Decommunalizing" the security services here implies a wholesale attack on one community's employment, which that community can be expected to resist. Weitzer's discussion of the transition from Rhodesia to Zimbabwe shows just how unusual and *accidental* the preservation of the white-dominated security services during the transition was. Because all the participants anticipated a victory in the 1980 election, they all tried to preserve the security services they assumed they would inherit. A more cautious approach to the negotiations would have dictated a preemptive "defanging" of those services, particularly by the minority whites and ZAPU.

Despite these flaws, Weitzer helps focus attention on a crucial internal determinant for successful liberalization and democratization. His analysis is particularly useful to students of South Africa, where the security services' role in fomenting tribal conflict and its connections to different political parties make clear the costs of having an uncontrolled state-within-a-state. It will also be useful to students of the (former ?) Soviet Union, where Russian "settlers" are a potent political and demographic force in many of the new republics.

University of Virginia,
Charlottesville HERMAN M. SCHWARTZ

INTERNATIONAL RELATIONS

ASEAN and the Diplomacy of Accommodation. By Michael Antolik. Armonk, NY: Sharpe, 1990. 208p. $39.95.

The Association of Southeast Asian Nations (ASEAN) is one of the least known regional organizations and is subject to considerable misinterpretation as well as general neglect. This book by Michael Antolik is both a history of its origins and an explanation of the members' perspectives on how the ASEAN serves each of them. As such, it is a welcome addition to the literature of both Southeast Asia and international organization. It is particularly useful in filling the "non-Vietnam" gap that has existed for Southeast Asia since the early 1960s. Since that time, attention has been so focused elsewhere that Malaysia is sometimes thought to be the entire archipelago and ASEAN is often thought to be a misspelling of *Asian*.

In addition to the important task of bringing the details of the ASEAN and its members' roles to our attention, Antolik's book makes a contribution by examining the purposes that the ASEAN serves, rather than its structures. He notes that these purposes have evolved. Early on it was to serve as "a face-saving and noncontentious instrument of war-termination diplomacy," with immediate successful application in the Confrontation period. The ASEAN has gone on to be a forum, in which its six members (Malaysia, Thailand, Indonesia, Brunei, the Philippines, and Singapore) have ready access to each other for discussions of bilateral problems. It has facilitated common stands on important regional and global issues so that the six can synergistically generate more power and speak with a single voice. The regular contact among the members has brought about mutual understanding and the discovery of many commonalities.

Antolik correctly points out that there has been little of the economic integration that was so highly touted at the origins of this and many other regional organizations. He suggests, however, that political cooperation has far exceeded what was expected in 1967, and he indirectly documents the bilateral military cooperation that might well not have occurred without the auspices of the ASEAN environment.

His argument that "progress toward cultural cooperation has been more dramatic" than that toward economic integration needs to be subjected to further research, however. The exciting technological progress of ASEAN members has kept them in contact with the rest of the world; and the same influences that are reflected in Yugoslavia's divisions, the Soviet Union's continuing disintegration, and the disarray in the Middle East are present in Southeast Asia. Cultural cooperation between Malay Muslims and the region's Chinese has not improved in any documentable way. Also, internal cultural divergences have reverberations in the bilateral relations of the member countries that do not suggest increased cultural cooperation. In fact, the situation looks much like it has looked for many centuries. What the ASEAN has done is provide a forum for alleviating the potential political and military problems that arise out of continued cultural cleavages. The cleavages, however, are still there and may even be growing.

Antolik's treatment of the colonial period's contribution to the ASEAN's origins and mission reflects the ambivalence that the member nations themselves often display. He seems to want to distinguish between domestic and regional circumstances in advancing the positions that colonialism is the source of regional disruption and that significant divisions existed prior to colonial times, while not fully recognizing that *domestic* and *regional* are not dichotomous circumstances in this region. Singapore was once domestic Malaysia, for example, and Indonesia was a region with many domestic parts. If the ASEAN had focused on resolving problems that are presumably a function of the colonial period, it would have had little of the success that the author rightly points out it has.

The strength of Antolik's book is that it draws attention to the communication and other functional successes of ASEAN, pointedly downplaying the integrationist and economics criteria that often characterize such works. His descriptions of both domestic and bilateral contributions of the ASEAN forum are particularly noteworthy and important. His histories of the circumstances under which each ASEAN member found solace or utility in the community are also interesting portrayals of the linkage between nation-building processes and the nature of the international environment. The reader will be well educated about the development of both the ASEAN and its component states. The book's only real weakness is in the author's good-willed intent to paint a positive picture of the future of economic and cultural cooperation in the region (p. 156). Fortunately, he has gone into great detail to explain how ASEAN serves its members well, specifically in controlling economic competition and deterring cultural disintegration. If we compare the ASEAN with other similar organizations, these low-profile but critical successes already demonstrate the worthiness of the association. We need not place on the ASEAN the burden of altering resource distributions and centuries-old patterns of social and political identity.

American Graduate School of International Management — LLEWELLYN D. HOWELL

Investment Behavior of Multinational Corporations in Developing Areas: Comparing the Development Assistance Committee, Japanese, and American Corporations. By Bret L. Billet. New Brunswick: Transaction, 1991. 161p. $32.95 cloth, $24.95 paper.

Multinational corporations have been the topic of numerous books and articles over the last several decades. Bret Billet takes an innovative approach by addressing the question "How do multinational corporations react to the environment that they find in less developed countries?" (p 15). In answering this question, he argues that (1) various economic and political factors help determine whether foreign companies invest in poor countries; (2) foreign investment behavior often is different for Development Assistance Committee (DAC), Japanese, and American multinational corporations; and (3) foreign investment behavior frequently varies by geographic region of the world, that is, different corporations have a greater propensity to invest in some areas than in others.

Although this book contains many good ideas, it relies heavily on a flawed quantitative analysis that diminishes its overall contribution. Because Billet stresses his empirical analysis, I will discuss four serious problems with it. First, contrary to his claim (p. 21), the statistical analysis is *not* really based on a lagged dependent variable model. This type of model should include a lagged dependent variable as an *independent* variable, thereby controlling for the prior effect of the endogenous variable. Although Billet measures his dependent variables at different time points, he does not include a lagged dependent variable in the right-hand side of the equation. This reduces the causal explanatory power of his model, a point which has been stressed in previous development research.

Second, Billet supposedly uses dummy variables to analyze regional differences in foreign investment behavior. He claims to examine subsamples of Latin America, Asia, and Africa by including dummy variables for region (p. 59, n. 53). Throughout the book, however, no equation *reports* a dummy variable coefficient for *region of the world*, confusing readers and forcing them to wonder what is happening. There are three possibilities. Billet may have restricted his sample to specific regions when conducting the regional analyses. This is not possible, however, because the sample size is still close to 90 for *each* regional analysis! Or Billet may have included an intercept dummy variable in the analysis but failed to report it, which would be incorrect methodologically. Even if reported, an intercept dummy variable does not measure his primary concern, namely, the potentially different effects of *each* independent variable in Latin America, Asia, and Africa. This can be tested by calculating a series of *slope* dummy variables—dummy intercept variable for each region times each independent variable of interest. These interaction terms *must* be reported in the relevant equations. Finally, Billet may have attempted some other procedure that is not clear from his discussion and tables.

Third, case selection is problematic throughout the analysis and warrants discussion by the author. Specifically, the multiple regression equations examining the determinants of DAC and American corporate investment generally are based on 80–90 cases (countries). Importantly, however, the regression equations assessing the determinants of Japanese investment are based on 38 or fewer cases because "the Ministry of Finance reported foreign direct investment figures for a limited number of less-developed countries" (p. 58, n. 49). This is problematic, because the book is based on a comparison of DAC, American, and Japanese investment, thereby requiring a common sample in order to draw reliable statistical comparisons. Results produced by a small subsample of countries that have Japanese investment data should not be generalized to the larger sample, which is used to assess DAC and American investment. At the very least, the analysis should contain some *additional* equations that utilize the subsample to explore determinants of DAC and American investment.

Finally, the indicators tested in each chapter explain very little variation in the dependent variables. By my count, this book contains 176 multiple regression equations, 119 of which have an adjusted R-squared value of .10 or less (often much less). Although the R-squared value is overemphasized by many social scientists, it still tells us something about the explanatory power of an equation. When the value is consistently near zero, it should compel the author to examine additional variables or explain the situation. For example, although Billet posits that political repression, human rights, and other factors lead to foreign investment, it is possible that the reverse ordering is true. Previous research has demonstrated that foreign investment may well contribute to a climate of political repression, suggesting that political repression should be the *dependent* variable in this instance.

In conclusion, this book contains many good ideas. It asks important questions and frames them in an interesting analytical and theoretical framework. Unfortunately, however, the book also stresses the statistical testing of its arguments, and these results are not convincing because of various methodological shortcomings. This is a serious problem because we cannot trust the findings "uncovered" in the data analysis, an analysis that would carry great promise if properly executed.

Indiana University, Bloomington YORK W. BRADSHAW

American Hegemony and World Oil: The Industry, the State System, and the World Economy. By Simon Bromley. University Park: Pennsylvania State University Press, 1991. 316p. $39.50.

The age of cheap, clean, abundant energy supplies is over. Domestic oil production has been on a decline since 1970. The nuclear power industry is nearly at a standstill. New technologies in coal and other fossil fuels promise higher prices, delayed deliveries, and a set of politically sensitive questions about externality costs and environmental risks. Yet the political recognition of these trends lags significantly behind.

Simon Bromley's new book explores this dissonance inadvertently and at times brilliantly. His main claim is that American control of oil still exists. Yet his own meticulous reconstruction of history shows that in 1950 the United States produced 40% of world output and 27.3% of world exports, in 1980 only 25% and 17% (p. 72). Now if *hegemony* means controlling 40% or more of world economic activity, S. P. Huntington and others have noted that American hegemony disappeared long ago. Once we acknowledge this, Bromley's book might be better titled *The American Insistence for World Oil: 1900–1990.*

In fact, it is when Bromley discusses oil as a strategic commodity, rather than as a physical supply, that his book proves most useful. The book documents the shift of leadership during the world wars in the Middle East from British to American; explores America's strategic involvements with OPEC in the 1970s; and defines our allies and rivals in Europe, Japan, and the Soviet Union along a petrochemical yardstick. Bromley reveals that what really matters is "the complex conjunction of trends by which U.S. control over world oil became a key resource in the overall management of its global leadership" (p. 86). His book, particularly the middle chapters, examines these trends with insight and a wealth of details.

Missing from this book is a sense of consequence—an awareness of what this strategic insistence on oil costs. Instead, Bromley states with a strange abandon, "Within the general global energy balance, oil remains the regulator of the world energy markets due to its versatility, convenience, and even cost" (p. 216). In his sixth and last chapter, he discusses America's "reconsolidation" of world oil in the 1980s as if the Exxon Valdez spill, the international meetings on global warming, and the long foreground of stresses that led to our most recent war with Iraq were mere sideshows, curious blips in a prerecorded message meant to repeat itself without question or interruption until worn smooth by reuse.

Other titles go much further in describing the costs of this strategic insistence. Three of the best are John Blair's *Control of Oil* (1976), Daniel Yergin's *Energy Futures* (1980), and Martin Melosi's broad energy history of the United States, *Coping with Abundance* (1985). According to present mineral reserve lifetime figures of the U.S. government, if current rates of consumption and reserves continue, the United States will run out of oil in 9.9 years, while the world supply will be depleted in 45.3 years. These are the indicators that guide the above titles, not simply the continued dominance of a worldview and its politics.

Nonetheless, the story Bromley reconstructs is an important one, full of astonishing facts and political anecdotes. It should be read by all Americans who already know that 80% of the oil wells drilled in the world have been drilled in the United States and those intrigued by the political consequences of having these three million domestic wells slowly run dry.

Rensselaer Polytechnic Institute BRUCE PIASECKI

China's Changing Relations with the Middle East. By John Calabrese. New York: Pinter, 1991. 183p. $49.00.

In several respects, John Calabrese has written an ambitious and useful book. China's attitudes and policy toward the Middle East remain one of the least explored subject areas in the literature of Chinese foreign relations. This book and Yitzhak Shichor's *Middle East in China's Foreign Policy* (1979) fill this gap in our understanding of China's changing relations with the Middle East.

The main objective of the book is "to map the changes and continuities in China's relations with the Middle East: to periodise the forty-year history of Sino–Middle Eastern relations" (p. 2). This exploration takes a chronological narrative format, describing China's policy toward the Middle East as comprising seven distinct developmental phases represented by seven chapters: "Leaning to One Side," "The Cost of Militancy," "Social Fortress," "Prelude to Pragmatism," "Thwarting Superpower Hegemony," "Leaning to the Other Side," and "Developing Influence and Influencing Development" (phases beginning in 1950, 1958, 1966, 1969, 1972, 1978, and 1982, respectively).

The author sees China's Middle East policy as reflecting the changes and shifts in Chinese foreign policy as a whole. Calabrese is at his best when providing an account of China's changing perceptions of the Middle East, from the theater of Sino–Soviet collaboration in the 1950s and the arena of Sino–Soviet rivalry in the 1960s, to the vortex of superpower contention and collusion in much of the 1970s, and an integral and increasingly important part of the globalization of the open door policy in the post-Mao era. He is less illuminating in depicting the continuities of Chinese foreign policy. Throughout the book, he repeatedly recalls the realist premise—that China has consistently interpreted Middle East developments in terms of the structure of global politics dominated by the superpowers—as accounting for the continuity of Chinese foreign policy. But this realist premise is not applied rigorously or systematically enough to add anything significant to the narrative.

Essentially, the book thus *assumes* the realist perspective. Beijing's Middle East policy over the years is said to have consistently followed a "strategic logic" defined in global, rather than regional, terms. Hence, Calabrese tacitly embraces the conventional wisdom in the study of Chinese foreign policy that China is a regional power without a regional policy. Much of the argument seems to echo Schichor's central thesis that China's Middle East policy can be best understood as a reaction to global events dominated by the superpowers, rather than to indigenous conflicts in the region. He uncritically accepts another realist premise—the state-centric rational actor model—as he asserts that Chinese foreign policy in general and Middle East policy in particular have enjoyed an elite consensus within the Chinese leadership.

The book provides a balanced and thoughtful examination of China's changing perceptions of, and relations with, Egypt, Iran, Iraq, Syria, Saudi Arabia, Kuwait, Israel, and the PLO. It is particularly impressive in providing fascinating background briefing on China's relations with Israel and the PLO.

Yet *China's Changing Relations with the Middle East* strikes me as disappointing on several counts. First, there is the problem of timing and coverage. Since the book is designed to "update Shichor's analysis" and "reconsider the question of China's role in the Middle East in the light of the decade that has elapsed since its publication" (p. 4), there is no compelling logic or justification in rehashing the entire forty-year history of Sino–Middle East relations, devoting only the last two chapters of a slim volume to the missing decade. It seems inexcusable that the author could not wait to take sufficient account of the post-Tiananmen developments, including China's involvement in the 1990–91 Gulf crisis, the most serious litmus test of Chinese policy toward this trouble spot. As a result, a book published in May 1991 was already made-to-order for historical anachronism, being replete with references to the "Gulf War" (meaning the 1980–88 Iran–Iraq War). The author himself (presumably unwittingly) committed this book to swim or sink based on its coverage of China's Middle East policy during the post-Mao era. The establishment of full diplomatic relations between China and Saudi Arabia in 1990 is another post-Tiananmen event that has not only rendered much of the analysis of China's relations with Saudi Arabia stale but also invalidated what little prognostication the book embodied. Second, the book is marred by an excessively sympathetic "Nixon-in-China" strategic realism. There is little probing of the theoretical and normative implications of the erratic twists and turns in China's Third World policy. Relying mostly on secondary sources and official pronouncements, Calabrese too readily accepts the rhetoric of independence at its face value, papering over the words/deeds—and even words/words—disjuncture between political independence and economic interdependence in the 1980s. Calabrese too readily accepts China's rhetorical identification with the Third World, failing to see and analyze the steady decay, since 1978, of China's moral regime and credibility as an antihegemonic champion of the Third World. As if to bridge the widening gap between policy pronouncements and policy performance, Beijing began to rationalize its decline as a Third World leader as proof that it has kept its pledge never to act like a hegemon.

Finally, the realist perspective seems to have led Calabrese to commit the fallacy of overstating external systemic factors and understating domestic factors in the making and execution of Chinese foreign policy. The notion that China's Middle East policy (especially arms sale policy) commands a consensus among the Chinese leadership is wide of the mark. The chronic gap between promise and performance on a range of global issues, most notably on arms sales and transfers of sensitive nuclear and missile technology to the Middle East—largely left undetected and unexplored in the book—is not entirely due to mendacity (although this, too, is a problem). Because of the rise of multiple power centers within the Chinese government in the 1980s, the Ministry of Foreign Affairs finds itself making international pledges and commitments that it has no power or authority to implement.

Princeton University Samuel S. Kim

The Dynamics of Domestic Politics and Arms Control: The SALT II Treaty Ratification Debate. By Dan Caldwell. Columbia: University of South Carolina Press, 1991. 234p. $32.95.

The weight of domestic politics on American foreign policy comes under detailed scrutiny in this case study of the nonratification (after seven years of negotiation) of the U.S.–Soviet Strategic Arms Agreement (SALT II) in the waning months of the Carter administration. Caldwell examines Carter administration approaches to foreign policy and arms control; its SALT II ratification strategy; the roles of major domestic actors (public opinion, interest groups, and the Senate); and the impact of external events (the Soviet-brigade-in-Cuba contretemps, the hostages in Iran, and the Soviet invasion of Afghanistan).

He deals rather thoroughly with two major explanations of nonratification (Carter administration shortcomings and Soviet bad behavior), less thoroughly with the substantive security merits of the treaty. Proposals for fundamental reform of American institutions by the losing treaty advocates are noted: a parliamentary, rather than presidential, system; a six-year, one-term presidency; and ratification by a majority of both houses of the Congress, rather than two-thirds of the Senate. In sum, both domestic politics and external events matter for arms control agreement ratification. Presidents need to pay much greater and much earlier attention to ratification strategies.

According to Caldwell, six lessons are derived from his case study. First, presidential failure to damp—indeed, almost to directly guarantee—lack of foreign policy consensus among key officials has adverse consequences. Major policy steps have strong opponents at all times in the administration itself. They build blocking coalitions with the Hill and the media. Potentially decisive institutions (in this case, the CIA or Department of Defense) cannot be used decisively to break deadlocks between the national security advisor and the secretary of state. More generally, mixed signals result that involve concessions to committed opponents of particular initiatives and slights to undecided and middle-of-the-road actors.

Second, on controversial matters, it pays to borrow legitimacy from previous presidents, especially ones of the opposition party. For instance, the early decision not simply to proceed along Vladivostok lines was a policy error. Beware of raising the threshold of objectives lest more modest steps become suspect. For example, the Jackson-backed comprehensive proposals to Moscow early in the Carter administration quite possibly led Carter into a trap set by opponents of détente.

Third, no one familiar with Washington during the Carter years could have been unaware of the systematic "Jackson mafia" campaign against both détente and anyone with possible influence who did not oppose it. Dealing effectively with such opponents involves changing the "face of the issue" or mobilizing proponents of equal zealotry and clout. The Carter administration made little attempt to do either.

Fourth, personal resentments and career motives will dominate all but the most dramatically meritorious policies. SALT II was not one of those. Nor is there any reason to believe that the institutional reform suggestions will in general offset more parochial considerations, especially shortly before elections. When policies involve personal costs, ask for the ones that matter most first because you will not get support for very many.

Fifth, Carter's tactics almost worked in spite of everything. SALT II was headed for ratification until foreign events knocked it off track. A faster timetable or a different process might have worked. Why did Carter not use available precedent to go for a majority approval by both houses of Congress? With continuing foreign cooperation, ratification might have succeeded. Where was the strategy to secure continuing Soviet and other foreign cooperation or to hedge against its not being forthcoming? Domestic strategy will not suffice.

Sixth, arms control, unlike arms reduction, focuses on national advantage and enhanced military power. It is not compatible with signs of U.S. decline. Ratification was lost when foreign events seemed to signal decline and an administration ambivalent about national power and national principles and fumbling. Simple principles and confidence increase presidential influence. The opposite traits undercut it.

The work is, in general, balanced and judicious. Yet several matters merit more attention than they receive. The reader looks in vain for explicit connections to an American policy world far different from that centered on U.S.–Soviet rivalry and mutual threats of extinction.

University of Pittsburgh Davis B. Bobrow

Core/Periphery Relations in Precapitalist Worlds. By Christopher Chase-Dunn and Thomas D. Hall. Boulder: Westview, 1991. 300p. $27.50 paper.

Sociology of the Global System. By Leslie Sklair. Baltimore: Johns Hopkins University Press, 1991. 869p. $42.00 cloth, $13.95 paper.

From perspectives that view the world as primarily a capitalist system but in a spirit that is increasingly ecumenical, two new books out of sociology raise issues of broad implications for the social sciences.

Leslie Sklair deplores the fact that "with few exceptions . . . social scientists have not yet come to regard the whole world as a legitimate object of knowledge." The difficulties attending such an undertaking are obvious, and attempts to derive universal propositions from comparative sociology have been unfruitful. To remedy this state of affairs, the author proposes a sociology of the global system that goes beyond the "state-centric" approaches of many students of international relations, a conception based on "transnational practices."

For Sklair, "sociological totality" is constituted by transnational practices that are analytically distinguished on three levels: economic, political, and cultural–ideological. In the world of today, "largely structured by global capitalism," these three practices are typically represented by three major institutions, namely, the transnational corporation, the transnational capitalist class, and the culture–ideology of consumerism, respectively: "Effective [transnational corporation] control of global capital and resources is almost complete" (p.).

This is a bold and simple idea, buttressed by fieldwork on export-processing zones (e.g., in China and Mexico); a wide reading of recent literature on multinational corporations, especially in the Third World; and much attention to the Second World. But it offers no more than a "thin description" of the "whole world," whose mysteries we are trying to unravel. Transnational corporations are indeed responsible for the bulk of international trade, but they employ less than 1% of the world's labor force and add no more than about 10% to the world product. To claim for them "effective control" of world capital and resources, as well as of politics and culture, seems to exaggerate unduly and to leave room for little else. In any event, time may be ripe for a new synthesis. The bulk of transnational corporation activities is now in the developed world. The Second World is in the process of dissolution, and the Third World is, consequently, also transforming. The source of difficulty seems to lie in Sklair's equating the global system, in whose economic elements the transnational corporation is indeed quite prominent, with the world system and the world economy, in which it is much less so.

Christopher Chase-Dunn and Thomas Hall's *Core/Periphery Relations in Precapitalist Worlds* is an effort to reinvigorate the world system theory by extending its reach in time. The editors of this collection of 10 pieces (including substantial contributions by two political scientists, David Wilkinson and Barry Gills, coauthor with André Gunder Frank) set out to place world system analysis on a new track, by discarding what have been some of the basic tenets: the singularity of the modern capitalist world economy, the axiom that the capitalist core exploits the periphery, and the strategic role of the trade in bulk commodities to that process. While "in no way endorsing a unilinear model of evolution," they propose a threefold typology of world systems (kin-based, tribute-based, and capitalist) and include a paper by Stephen Sanderson, who denies any "inherent antagonism between an evolutionary and a world-system perspective."

Such ecumenism is welcome. It might be signaling the waning of the "age of paradigms" and the return of the search for truth. It also reflects the growing conviction that the social sciences, in their quest for a full grasp of the human experience, cannot be confined to the last five hundred years and need to extend their sights to maybe at least five thousand years of world system history. But the book itself does not point to a clear road ahead. Chase-Dunn and Hall themselves advocate a "variation-maximizing," comparative research program on intersocietal inequalities among different sorts of world systems, including stateless societies.Their contributors Gills and Frank argue for a continuity-emphasising study of the "one, cumulative" world system leading up to a "comprehensive world system history" that is "humanocentric." I opt for the latter.

University of Washington GEORGE MODELSKI

Irredentism and International Politics. Edited by Naomi Chazan. Boulder: Lynne Rienner, 1991. 161p. $26.50.

With the breakup of the Soviet Union and the Cold War officially declared at an end comes a well-timed book on the subject of irredentism and international politics. Irredentism—a state's attempt to redeem foreign-controlled territory containing ethnically similar peoples—is a concept that has not received a great deal of examination or discussion within the field since the period of decolonialization began, following World War II. Irredentism is a latent phenomenon that normally only manifests itself during periods of massive transformation in the international system—as with the establishment of the new European states following World War I, the process of decolonialization following World War II, or the end of the rigid bipolarity of the cold war era. Contemporary irredentist issues are evidenced by Poland's cautious acquiescence in the reunification of the German state, Iraq's invasion of Kuwait, and the newly installed Khadafi-aided regime in Chad. All point to the resurgence of irredentist claims and the role of irredentism in international politics.

One of the major reasons for undertaking a reexamination of irredentism is that because of its intermittent manifestations and lack of attention from the field of political science, it lacks conceptual clarity. The term *irredentism* has been mistakenly used in the past to describe any form of territorial dispute. On the other hand, it has also suffered from overly restricted usage. Crawford Young states in his *Politics of Cultural Pluralism* that "despite the historical artificiality of most state units in the third world, Somalia is the base for virtually the only culturally based irredentist movement" (1976, 95). Most scholars within the field of ethnicity and nationalism would take issue with this statement. Benyamin Neuberger devotes his entire essay in Chazan's volume on the intricacies of real and potential irredentist claims on the African continent. Young's statement does illustrate, however, the lack of a precise understanding of what is meant by *irredentism*.

Milton J. Esman defines *irrendentas* as "territorially-based minorities contiguous to a state controlled by their co-ethnics" ("Ethnic Pluralism and International Relations," *Canadian Review of Studies in Nationalism* 17 [1990]: 83). The concept of irredentism, he argues, is closely related to the doctrine of self-determination of peoples; but (as his definition suggests) it is the state and its elites that seek to reclaim irredentas.

Although Chazan and the other authors in this collection seek to establish a concrete definition of irredentism and to clarify some of its conceptual ambiguity, their discussion leads only to a further muddling of the concept. This is particularly the case with the essay by Donald L. Horowitz, who seemingly blurs the distinction between the right to national self-determination and irredentist claims. Horowitz maintains that beside the statist element recognized by Esman, irredentism also consists of "the attempt to detach land and people divided among more than one state in order to incorporate them in a single new state," citing the formation of a Kurdistan as an example (p. 10). The Kurdish call for the creation of their own independent state is an example of a nonstate nation's attempt to exercise its right to self-determination. It is not, however, an example of irredentism.

In her introductory chapter, Chazan states that this examination of irredentism was undertaken to "shed light on the key facets of irredentism in comparative and historical perspective" (pp. 1–2). She argues that irredentism is conceptually distinct from separatism and that irredentist claims are more than mere territorial disputes. The Argentine claim upon the Malvinas/Falklands could not be considered irrendentist because "the islands do not contain an Argentinian population" (p. 3). Unfortunately, Chazan, like Horowitz, seeks to attach a "second variant" to the concept of irredentism, in which "components of multiethnic states have irredentist claims aimed at creating entirely new nation–states" (p. 2). Again, Chazan seems to be addressing here the right to the self-determination of peoples or perhaps even separatism, concepts that are related to, but distinct from, irredentism.

Not all of the authors of this collection fall into the same trap as Horowitz and Chazan in seeking to meld two distinct concepts. Neuberger, in a highly competent and instructive discussion of the dynamics of African irredentism, recognizes the role of the state in launching irredentist claims. He states that "we must make clear that irredentism means an attempt made by an existing state to 'redeem' territories and peoples it considers its own" (p. 97).

In her concluding chapter, in light of the discussion within the eight essays that make up this text, Chazan offers a synthesized redefinition of *irredentism* that (finally) concedes the role of the state: "Irrendentism . . . refers to attempts by existing states to annex adjacent lands and the people who inhabit them in the name of historical, cultural, religious, linguistic or geographic affinity" (p. 139). In light of this definition, however, I am confident that I could still introduce Chazan to an Argentinean friend or two who would like to discuss with her the merits of Argentina's irredentist claims upon the Malvinas.

Taken as a whole, this is an extremely well-timed book in light of issues in contemporary international politics; and it seeks to address issues that lie either at the heart or in the rhetoric of interstate conflict—issues that have received scant attention from the field as a whole, with the exception of those who focus upon ethnic group relations and nationalism.

University of Arizona THOMAS A. HOPKINS

Debt and Crisis in Latin America: The Supply Side of the Story. By Robert Devlin. Princeton: Princeton University Press, 1989. 320p. $35.00.

Given the vast literature spawned by the international debt crisis of the 1980s there is now an added burden of originality applicable to any new monograph on the topic. Robert Devlin discharges that burden impressively in the present volume. While most analysts have focused on the role of both misguided debtor policies and 'northern' macroeconomic conditions in the creation of the crisis, few have subjected the creditor banks and the conditions of loan supply—what Devlin calls 'the supply side'—to sustained examination. Central to this lacuna are certain myths which have obscured the role of the banks in the genesis and management of the crisis. Absorbed by the possibilities of internationally unregulated private credit in the 1970s, an influential section of northern opinion came to view the banks as virtual 'freedom fighters' restoring classic laws of capital allocation on a global scale and enforcing financial discipline on a largely profligate South. Subsequently, when key debtors fell into defacto default the same opinion-makers largely absolved the banks of responsibility and endorsed the forced adjustment of debtor states by the compression of internal demand.

Devlin's central concerns are to critically evaluate this worldview, and to highlight the coresponsibility of the banks in the genesis of the debt crisis, their success in pushing the bulk of its costs onto the debtor states, and the gravity of the resulting situation in Latin America. He elaborates these concerns in five ways. First, he portrays the growing incentives for the U.S. money-center banks to go abroad from the 1950s. Second, he constructs an eclectic theory of loan supply to Latin America. From modern portfolio theory he draws insights as to how profit differentials influenced the drift of banking activity from domestic to international markets, and using industrial organization theory he highlights the oligopolistic character of international banking especially the leadership role of the key U.S. banks. This institutional context, Devlin contends, led the industry leaders to focus excessively on non-profit objectives and created a 'bandwagon' attitude to lending among the smaller banks. It also encouraged a high degree of permissiveness in loan approval and refinancing. Devlin illustrates his theory by constructing detailed and persuasive interpretations of the Bolivian and Peruvian cases. He concludes by advancing a relatively novel view of the crisis: namely, that the banks displayed a strong tendency to overlend in the upswing of the credit cycle and to overcontract on the downside. In this his work bears a close affinity with that of Hyman Minsky and other theorists of financial crisis.

An analysis of the banks' bargaining stategies during the repeated rescheduling negotiations of the 1980s forms the third element of the study. Devlin argues that the banks, buttressed by both the key OECD states and widely shared precepts of financial orthodoxy, effectively formed a creditor's cartel through which they shifted most of the costs of the crisis to the debtors. While this argument is not new, Devlin's achievement is to give it a highly persuasive analytical and empirical cast. The fourth element of the inquiry is a critique of the forced adjustment imposed on the debtors. For Devlin, the resultant outward transfer of resources from Latin America, nearly one-fifth of the region's domestic savings, has been a major disaster. The excessively short-term focus of adjustment programs, the serious erosion of the debtor's productive base they induced, and the inadequacy of the compensatory finance flowing to Latin America have all created a vicious cycle of economic and political instability. Arguing from a Keynesian perspective, Devlin contends that any socially efficient adjustment should target the debtor's capacity to pay, and, in turn, transfer resources abroad only out of trade surpluses which reflect a rise in savings and tradeable goods. He is scathing of the I.M.F.'s failure to balance the provision of lender-of-last-resort facilities for the banks with socially efficient adjustment for the debtors. The final part of the study considers options for change. Anticipating the most recent policy debates, Devlin argues for debt forgiveness, the importance of multilateral debt initiatives, and adequate public finance for growth.

Foremost among the strengths of this volume is the theory of loan supply which insightfully synthesizes approaches across perspectives. This is complemented by rich empirical material and a subtle, nascently post-empiricist methodology. It is, however, open to at least one major criticism. While Devlin marshals a vast amount of material on the role of the banks, state elites and ideas in the episodes he considers, he fails, even briefly, to theorize the wider politico-economic context within which the banks acted and bargained. Why, for instance, have the banks enjoyed so much power with governments in matters of international banking over the last two decades, and what does this tell us about developments in both the direct and indirect power of business especially transnational finance over this period?

This criticism aside though, Devlin's study is a subtle and judicious one which has made an important contribution to both an under-researched perspective on international debt issues and a growing literature on the politics of creditor coalitions in the international political economy.

University of Sydney IAN BELL

George Ball, Vietnam, and the Rethinking of Containment. By David L. Di Leo. Chapel Hill: University of North Carolina Press, 1991. 220p. $37.50 cloth, $12.95 paper.

David L. Di Leo has produced a well-written and thoroughly researched volume on one of the more influential American leaders of the cold war years, George Ball—a man who has been called a "curmudgeon" and would most likely agree with the description. Di Leo even titles one of his chapters describing Ball, "A Wholly Partisan Critic."

Di Leo traces the wide sweep of Ball's career through government work, dabbling in politics and the business and legal professions. Ball began his varied career in the 1930s, just as storm clouds were looming over Europe and formally ended it in the 1980s, just before the formalities of the forty-year-old cold war began slowly coming to an end. Ball began as something of a political outsider at the beginning of the Kennedy administration; but through the help of his friend and mentor Adlai Stevenson, he was appointed to the number 2 position in the State Department as undersecretary for economic affairs.

Ball is perhaps best known for his involvement in the debates on American policy towards the Vietnam War. These debates began as early as 1961, even though (as Di Leo notes) other areas of the world were actually of greater interest to Ball than Southeast Asia. By 1961 Ball had already emerged as an opponent of any further American action in Vietnam based both on his broader interests (in his judgment, other areas of the world were much more important to the United States at the time) but also because of his reading of the defeat of the French in 1954. Even at that early stage, it was clear that Ball was in the minority.

These debates continued and became the fulcrum point for the Johnson administration in 1964 and 1965. The familiar arguments were put forth by many: the so-called commitment by the United States to South Vietnam under Eisenhower, the "need" and the "vitalness" of South Vietnam for United States national security interests, and the analogy with Korea (i.e., aggression must be stopped). Di Leo presents a very detailed account of these debates, picturing Ball as an extremely articulate player although very much in the minority—primarily because key players Johnson, Rusk, and MacNamara very much favored the early-1965 bombings and the further prosecution of the war itself. Of note about these debates, however, is the way at least some of the players could be simultaneously "antagonists and allies"—disagreeing on Vietnam but not necessarily allowing that to spill over into other areas. Needless to say, such a pattern did not necessarily apply to all the players.

Fortunately, Di Leo goes beyond the rather strict confines of a historical narrative that most of the book is devoted to. He also places Ball—especially in terms of the arguments over the Vietnam involvement but also in the larger argument about containment as a policy—into the broader context of the theoretical debates of the cold war years. While "realism" was the linchpin of many of the justifications for the containment, he notes that the "radical revisionist" school opposed many of the tenets of realism and that policy prescriptions derived from such a school therefore differed radically from many of the policies derived from containmnt. Here, "the rethinking of containment" comes into the picture, because these debates were the first time that any of the tenets of traditional "containment" policy as manifested in the Vietnam decisions were brought into question.

Di Leo's book is certainly detailed in terms of historical argument and presentation. In fact, therein lies one of the minor criticisms, namely, that it is often difficult to separate the wheat from the chaff. What is important, and what is not? Which meeting is important, and which is not? Second, while subtly critical of Ball at various points, Di Leo's admiration clearly shines through, and one wonders how much positive bias this might have rendered. Third, it is not clear exactly how Di Leo's book fits into the broader literature on the Vietnam conflict for there is little systematic citation of it. Fourth, the findings and evaluations presented throughout the book are sometimes confusing because so extensive. It is as if the reader is continually treading water, not knowing whether the end of this paragraph or the next or the end of the chapter will bring some relief, conclusion, evaluation.

Going beyond these somewhat narrow comments, however, two other observations can be made. First, Vietnam was not of major concern to Ball. Di Leo makes it quite clear that other areas of the world (especially Europe, with a focus on France and England) were of much more interest to him. As Di Leo notes:

> An estimate derived from his appointment calender during his six-year tenure as under secretary of State shows that he spent perhaps as little as 5 to 10 percent of his time on Vietnam. Save for intermittment periods when he was thoroughly consumed by the subject, the Europe-focused undersecretary mostly busied himself with the world far to the north and west of Indochina. (p. 153)

This leads to the second observation: Why was this the case? If 90%–95% of Ball's attention was devoted to non-Vietnam issues, why is he remembered mostly for his dissenting views on Vietnam? Is it because he was one of the few dissenters on the subject? Or is it because, in fact, at root, he was—or played—the curmudgeon? But curmudgeons are often known to alienate those they are "curmudgeoning" against. One is then led to ask the broader question whether, if Ball had eschewed this role during the Vietnam debates (especially since his true interests were apparently elsewhere), he might have had a much more direct and positive impact on the debates? It is of note that his role in history will most likely hinge around the Vietnam debates, while his true interests were apparently elsewhere.

Despite these clearly speculative observations, Di Leo's book is extensive and well researched. Anyone interested in the internal Vietnam debates of the early 1960s, in the give-and-take that goes on in many internal debates, and (obviously) in George Ball, this is must reading.

University of Arizona MICHAEL P. SULLIVAN

Conflict and Intervention in Africa: Nigeria, Angola, Zaire. By Herbert Ekwe-Ekwe. New York: St. Martin's, 1990. 195p. $59.95.

The Superpowers and Africa: The Constraints of a Rivalry, 1960–1990. By Zaki Laidi. Chicago: Chicago University Press, 1990. 232p. $45.00 cloth, $14.95 paper.

The Nonsuperpowers and South Africa: Implications for U.S. Policy. By Richard J. Payne. Bloomington: Indiana University Press, 1990. 314p. $35.00.

Middle Power Internationalism: The North–South Dimension. Edited by Cranford Pratt. Montreal: McGill–Queen's University Press, 1990. 167p. $32.95.

Given the tremendous changes occurring in the structure of international relations, particularly the collapse of the Soviet empire, the need to focus greater attention on the foreign policies of countries other than the two superpowers is slowly being recognized. Three of the four books reviewed here are of particular interest for their contribution to the less well developed literature on the interests, policies, and actions of nonsuperpowers.

Cranford Pratt's book focuses on the failure of the South to achieve any significant reforms of the international economic system. In the introductory chapter, Pratt reviews the debate of the 1960s and 1970s over the creation of a new international economic order based on "humane internationalism." He argues that there is a "carefully reasoned theoretical and ethical foundation for humane internationalism" and presents three possible methods for its implementation: radical internationalism, reform internationalism, and liberal internationalism (p. 8). However, despite some support for a more equitable international order, most leading policymakers in the developed world have accepted too readily the principles and assumptions of the school of realism. According to Pratt, realism pushes states "to enhance their power and their influence in order better to advance their interests," which eventually "becomes a self-fulfilling prophecy" (pp. 11, 13). While deploring the policies and shortsightedness of the North, the book focuses on the possible role that middle powers (five in particular) might play in persuading the countries of the North to accept humane internationalism as a priority.

In the three chapters that follow, Asbjorn Lovbraek, Bernard Wood, and Raphael Kaplinsky examine the possible contribution of middle powers to alleviating the poverty and underdevelopment of the South. Lovbraek presents a lucid explanation for the inability of the "like-minded group of middle powers led by Norway and the Netherlands to gain support for redistributive policies toward the South. His reasons are three: (1) the premises of the social welfare state at the domestic level could not be convincingly transferred to the international level; (2) the end of the 1970s saw the increase of economic problems in the North, which led to growing calls for protectionism; (3) the philosophy of reform internationalism was accepted by only a few government officials, limiting its appeal even within progressive middle powers.

The chapter by Bernard Wood, although interesting in its conceptualization, ends with some rather limited conclusions. Instread of only 5 middle powers, Wood expands the definition of middle powers to include 33 countries based on both quantitative and qualitative measures (gross national product and influence, respectively). After presenting an overview of past experiences of middle-power action, Wood argues that "there is scope for co-operative middle power efforts in the international economy" while admitting that middle powers, in the past, have "seemed to be almost as locked into patterns of immobility as other states" (pp. 81, 89). Wood goes on to ask a sometimes overlooked question: What impact will the decline of U.S. hegemony have on middle powers? Although his conclusions are carefully qualified, Wood suggests that international cooperation among middle powers is possible but unlikely.

Kaplinsky, in the third chapter, analyzes the impact of microelectronic technologies used by the newly industrializing countries in Asia on middle powers. He argues that as the newly industrializing countries become increasingly competitive with the middle powers in certain industries, unemployment (in the latter group) will resist easy solutions, and conflict between the two groups will increase. The result will be an increase in protectionism or "a restructuring towards more managed patterns of trade" (p. 139). Kaplinsky's premise, that the newly industrializing countries have been able to compete successfully with middle powers, contrasts with the basic argument of the rest of the book that the South will never achieve economic progress without a new international economic order.

Pratt's study of middle powers and humane internationalism is important for its treatment of a topic that has received little attention in the literature. The difficulties in persuading the North to restructure its economic relationship with the South are well elucidated but should come as no surprise. Unfortunately, the book suffers somewhat from a lack of conceptual clarity and may appeal less to a scholarly audience than to sympathetic policymakers. Although Pratt's strident criticism of the North may seem one-sided to some readers, his opinions are clearly presented and are worthy of consideration.

Richard Payne also focuses on nonsuperpowers. He analyzes the policies of Britain, West Germany, Japan, Israel, Scandinavia, Canada, and the frontline states toward South Africa. No explanation is given for why he included these states and excluded such nations as France, India, and Australia. In his opening chapter, Payne discusses the factors that combine to influence the relations between the nonsuperpowers and South Africa. They include economic interests, ideological and geopolitical interests, domestic racial problems, military interests, domestic public opinion, relations with Africa and the Third World in general, and the (Former ?) threat of the Soviet Union. After assessing the "new realities" in South Africa, including political and economic developments, as well as the growing internal and external opposition to apartheid, Payne discusses two possible strategies that the nonsuperpowers might employ to press for the abolition of apartheid in South Africa: economic sanctions and black empowerment.

In each of the next seven chapters, the author examines the constellation of factors that have influenced each nation's foreign policy toward South Africa. Relying exclusively on English-language sources, he discusses the strategies (sanctions and black empowerment) implemented and outlines the nonsuperpower's policy toward the frontline states (those that border South Africa). Each chapter precisely follows this outline, which helps the reader make comparisons and assess the arguments. In addition, the chapters are cogently written and present an excellent starting point for further study.

Of particular interest are the chapters on Britain, Israel, and the frontline states. According to Payne, although Britain's foreign policy is largely a product of its economic ties to South Africa, other interests cannot be completely ignored. Trade between South Africa and Britain is important (British exports stood at $1,156 million in 1987), and Britain represents one of the largest sources of foreign investment. This economic link, when combined with the British government's claim that trade with South Africa protects jobs in Britain, has predictably led to an unwillingness to enforce severe sanctions against South Africa. Britain did partially agree to a European Community sanctions package in 1986, but the sanctions were rendered ineffectual due to the lack of British and West German support. Instead, the Thatcher government argued that economic sanctions would be counterproductive, because they would create greater self-reliance, reinforce the extremist Right in South Africa, and lead to greater unemployment for black South Africans. Payne concludes that British leaders may not approve of apartheid but that "they have no inclination or incentive to disturb a profitable relationship" (p. 58).

Israel, unlike any of the other countries discussed, bases its foreign policy almost exclusively on national security. In response to the 1973 war and declining African and European support, the Israeli government decided to cooperate with South Africa despite Israel's express condemnation of racism. This cooperation has been reinforced by technical and research collaboration, Israeli trade and investment activities, and the disinterest of Israeli public opinion.

Like Israel, the frontline states also pursue contradictory policies toward South Africa. Payne explains that there is a "chasm between Pretoria's neighbors' rhetoric against apartheid and their reality of extensive economic interdependence with the white minority regime" (p. 202). Economic factors dominate the foreign policy calculations of the frontline states. Dependence on South Africa takes many forms, including employment of frontline states' workers and the purchase of agricultural products, oil, and electricity. At the same time, however, South Africa is also dependent on the frontline states for markets for its manufactured products. This economic interdependence has led the frontline states to harshly condemn the apartheid regime, while at the same time opposing sanctions against South Africa.

In his conclusion, Payne argues that the United States should pay closer attention to the foreign policies of the nonsuperpowers toward South Africa; and he suggests a series of measures that the U.S. government should implement. Although his arguments may have merit, his brief discussion of the implications for U.S. foreign policy appears unrelated to the seven core chapters and adds little to the rest of the book. Another conceptual framework such as a theoretical or comparative analysis of the strengths and weaknesses of nonsuperpowers would have been more effective. Overall, Payne's study is well written and clearly formulated and fills a gap in the comparative foreign policy literature.

Herbert Ekwe-Ekwe has written a short but incisive book whose aim is "to investigate the nature of civil conflict and intervention in Africa with special reference to Nigeria, Angola, and Zaire" as representing the three dominant types of national liberation movements (p. 1). While not ignoring the presence of the two superpowers in each conflict, Ekwe-Ekwe also investigates the role of the Europeans (primarily France, Great Britain, Belgium, and Portugal); contiguous states; and regional and other local interest groups. The strength of his case studies rests on his insightful analysis and inclusion of all the important forces in each interventionist regime, rather than on a detailed presentation of the unfolding of each conflict and intervention.

After a short introductory chapter presenting the five major themes to be examined in each case study, Ekwe-Ekwe analyzes each of the three countries separately (chaps. 2–4). In the case of Nigeria, he argues that Great Britain played a key role in determining the outcome of the struggle between the Nigerian federal government and Biafra through its arms deliveries to Lagos. The Biafran call for secession was considered a threat to Britain's widespread economic interests in Nigeria (£1.5 billion), and this concern led the British government to side with the federal government, which supported the status quo. The Soviet Union, most African states, and the Organization of African Unity also favored the continuation of federal Nigeria, albeit for different reasons. Although France and Portugal gave some support to Biafra and the United States maintained a questionable neutrality, Ekwe-Ekwe argues that their limited response only served to postpone the inevitable collapse of the Biafran resistance.

In the case of Angola, the two superpowers, along with Cuba and South Africa, intervened to protect their perceived political, economic, and strategic interests. For the United States, the fear that the Soviet Union might gain a foothold in Africa and the desire to protect U.S. economic interests were the primary motivations behind its support for the (FNLA–UNITA) front. South Africa worried that the collapse of Portuguese rule in

Angola, in conjunction with a (MPLA) victory, would threaten its strategic and economic interests. These considerations eventually led South Africa to intervene militarily on the side of the FNLA–UNITA forces in Operation Zulu. U.S. and South African support for the FNLA–UNITA forces, however, was matched by Soviet and Cuban support for the MPLA. According to Ekwe-Ekwe, Soviet assistance through arms deliveries was motivated primarily by ideological reasons, not (as others have argued) strategic ones. He also asserts that Cuba's decision to intervene militarily (Operation Carlota) was made independently of the Soviet Union. Both of these assertions have been the subject of much debate, and Ekwe-Ekwe's rather brief discussion does not provide the definitive or final word.

In chapter 3, the author discusses the two attempts by the National Front for the Liberation of the Congo (FNLC) to take over the province of Katanga (presently Shaba). On each occasion, the FNLC was easily defeated for several reasons, the most important being the intervention by France and Belgium. France and Belgium had extensive economic interests in Zaire and considered the federal government under General Mobutu to be a better protector of the status quo. Egypt, Morocco, and the Sudan also supported Mobutu, while Cuba and the Soviet Union chose not to become involved. Ekwe-Ekwe concludes that external intervention was decisive in the rapid termination of the Shaba insurrections.

In chapter 5, Ekwe-Ekwe, in a somewhat confusing outline form, compares the essential features of the three interventions. While some of the propositions and assessments are of considerable interest others are both obvious and repetitious. Although this chapter would have benefited from a more concise and better-organized presentation, the analysis and conclusions, in general, remain sound. In sum, Ekwe-Ekwe has written a thought-provoking and convincing book on the critical importance of external and extracontinental forces on conflicts in Africa.

Zaki Laidi has written a scholarly study of the superpowers and Africa from 1960 to 1985 (despite the title) that merits considerable attention. In chapter 1, Laidi argues that the logic of the cold war and the struggle between the two superpowers was extended to Africa after World War II. Within a diplomatic–strategic framework, each sought both to check the other and to convince the African countries of the correctness of its ideology. Laidi argues that the United States and the Soviet Union did not achieve an understanding of the unique characteristics of Africa but, rather, projected "an eminently ethnocentric vision onto the African continent" (p. 5). However, despite concerns over communist advances, the United States did not consider Africa either "in the first perimeter of defense" (p. 31) or worthy of major economic investments.

In chapter 2, the author considers the possibility that Africa was of marginal interest to the two superpowers from 1965 to 1974. In the mid-1960s, apparent constraints on the power of the Soviet Union forced a choice between activism in the Third World and military parity with the United States (p. 32). The United States, increasingly involved in Vietnam, was also forced "to hierarchize its global objectives and relinquish some of its obligations to its Western allies" (p. 33). Given Africa's resistance to taking sides in the major East–West conflicts, these developments further diminished the interest of the superpowers in the continent.

The Angolan Crisis of 1975 (chap. 3) represents a turning point, or "line of fracture in relations between the great powers and Africa" (p. 59). Laidi discusses in some detail the ethnic divisions within Angolan society and the process by which the conflict became internationalized. In an analysis that complements Ekwe-Ekwe's chapter on Angola, Laidi outlines the process by which the Soviet Union and Cuba gradually became staunch supporters of the MPLA, while the United States and China stood behind the FNLA–UNITA forces. Unlike Ekwe-Ekwe, Laidi's analysis of Cuba's "independent" role in the conflict is more nuanced. He argues that without Soviet support, "Cuba never could have carried out an operation of such magnitude" (p. 72).

One important implication of the Angolan crisis was the "active integration of the African system into the East–West framework" (p. 74). This meant that each superpower, albeit in different modes, became more involved in the internal dynamics of its African "allies." The Soviets continued to place primary emphasis on the diplomatic–strategic arenas, while the United States also was anxious to preserve the "direct economic interests whose relative importance it had discovered" (p. 75).

In chapters 4 and 5, the author delineates American and Soviet foreign policy toward South Africa from 1975 to 1985. His analysis covers a broad spectrum of topics including U.S. diplomatic efforts to solve the crises in Zimbabwe and Namibia, the "missassessment" by U.S. policymakers in the Horn of Africa, and the change in U.S. aid policies beginning in 1976. Laidi argues that despite Carter's rejection of an absolute bipolar world, in general, the United States continued to view Africa in an East–West context, with some attention to economic interests. Interestingly, U.S. attempts to manipulate the content and direction of politics in Africa were often thwarted by complex African and European interests. For example, Kissinger's plan to solve the conflict between the white minority regime and the black majority failed dramatically due to opposition from both the frontline states and internal liberation movements.

Like the United States, the Soviet Union also experienced diplomatic and strategic reversals in Africa. Laidi argues that Soviet policy, based on the dual concepts of *a power like others* and *an ally in protest*, did not lead to substantial gains in influence. Despite numerous treaties of friendship and cooperation and a large degree of military assistance, "Soviet economic cooperation does not offer African states any clear advantage" (pp. 191–92). The "poor economic complementarity of the Soviet Union and Africa" and the "parsimony of Soviet bilateral aid" (p. 179) were key factors in the maintenance of ambiguous and precarious alliances.

Laidi concludes his study by arguing that Africa is suffering from a "loss of status" resulting from "lopsided integration" and a "new hierarchy of values in the world system"—both of which are detrimental to Africa (p. 196). In addition to the economic decline of Africa in the 1980s, as witnessed by the seriousness of the debt crisis, the superpowers have also reassessed their worldwide interests. Both the Soviet Union and the United States became less interested in ideological support and more interested in the reduction of costs. Both goals have served to diminish superpower interest in, and commitment to, the African countries.

The strengths of *The Superpowers and Africa* are manifold. The actions, motives, and implications of superpower involvement in Africa are presented in detail. Each chapter (except the conclusion) is well organized and makes good use of primary sources to support a perceptive analysis. The final chapter, however, suffers somewhat from a lack of relevance. It is strange that Laidi would conclude with a discussion of the implications of French decline for Africa, a topic untouched upon in any previous chapter. Unfortunately, despite the title, Laidi does not examine the implications for Africa of the dramatic changes occurring in the Soviet Union beginning in 1985 or the developments in South Africa. Although there are omissions, they do not radically detract from the central arguments of the book. This book will be of interest to the specialist, but its complexity will render it less accessible to the nonspecialist or general reader.

Old Dominion University PIA CHRISTINA WOOD

A Substitute for Victory: The Politics of Peacemaking at the Korean Armistice Talks. By Rosemary Foot. Ithaca: Cornell University Press, 1990. 273p. $32.50.

What if President Bush had agreed to negotiate with Saddam Hussein during Operation Desert Storm? Although this very good book is about the beginning of the Cold War in Asia and the end of the Korean war, reading it in the aftermath of the Guld War considerably enhances its relevance to the study of international relations and diplomacy today. It is an excellent text for courses in history, as well as in the politics of foreign policy decision making. Foot explains the perils of pursuing a policy that sought to gain at the conference table what could not be achieved on the battlefield. She also convincingly demonstrates how difficult it is to negotiate while fighting. Taken together with her earlier volume, *The Wrong War* (1985), the author has produced the best work of the 1980s on the Korean war and in a way that should appeal to readers whose interests extend well beyond this particular period of history.

The parallels between Korea and the Gulf are startling. Both wars began with an act of clear-cut aggression. They were costly, nasty conflicts in which civilian populations suffered enormously. The United States fought in both as part of a multinational coalition sanctioned by UN resolutions but on behalf of allies who saw the stakes of the conflict in far less global terms. The adversary was vilified to the point where it seemed almost inconceivable that an honorable settlement could be achieved by diplomacy. And both wars generated broad and substantial consensus on the need to punish such aggression regardless of how and when a deal could be struck.

But the central message here is that under such circumstances negotiations, if they take place at all, are likely to be protracted, frustrating, and replete with "tentative opportunities to end the conflict . . . left unexplored" (p. 217). Diplomacy, the author points out, was not a substitute means for compelling the aggressor to withdraw. A war had to be fought and virtually won. But the exigencies and emotions associated with the fighting also produced the tendency to obscure and minimize the chances to make a settlement sooner than the one that was signed in a tent at Panmunjom, some two years and 575 meetings after the negotiations started; for, according to Foot, both sides tended to treat each point on the agenda as if it were a piece of strategic territory to be attacked or reclaimed, rather than a basis on which to base a political settlement. This exceedingly well written and thoroughly researched book reminds us that without a policy process to coordinate military and diplomatic strategies, neither fighting nor negotiating are sufficient to produce victory.

This was the lesson of Korea. A decade later we failed to apply it in the case of the Vietnam War. And the Gulf War ended so quickly that no one can yet be sure whether it has been properly learned.

Georgetown University ALLAN E. GOODMAN

World Justice? U.S. Courts and International Human Rights. Edited by Mark Gibney. Boulder: Westview, 1991. 175p. $39.95.

Federal Courts and the International Human Rights Paradigm. By Kenneth C. Randall. Durham: Duke University Press, 1990. 295p. $45.00

International Fugitives: A New Role for the International Court of Justice. By Barbara M. Yarnold. New York: Praeger, 1991. 149p. $37.95.

In 1980, a Paraguayan doctor named Joel Filartiga and his daughter Dolly, who were both living in the United States, where they had applied for political asylum, brought suit in New York district court against the former Paraguayan police chief, Americo Pena-Irala, having learned of his presence in New York. Pena-Irala had kidnapped Dr. Filartiga's son in Paraguay and had tortured him to death to silence Dr. Filartiga's outspoken criticism of the Stroessner regime. The district court dismissed the case, ruling that torture of a Paraguayan in Paraguay by a Paraguayan official did not violate U.S. law.

The Second Circuit Court of Appeals overruled and held a little-known federal law, the Alien Tort Claims Act (28 U.S.C. sec. 1350 [1789]), applicable law in this case. Under the Alien Tort Claims Act, aliens are permitted to sue other aliens for violations of international law in U.S. federal courts. Since the United States had not ratified any of the major international human rights treaties, this landmark case opened the door for the prosecution of international human rights cases in U.S. federal courts. Whereas a number of cases have been brought under the Alien Tort Claims Act, the Bush administration has recently let it be known in an amicus brief in a California federal court action pending against former Philippine President Marcos that it does not view this statute as applying to cases presented by aliens against other aliens for human rights violations.

Mark Gibney's edited volume *World Justice?* and Kenneth Randall's *Federal Courts and the International Human Rights Paradigm* examine the role of U.S. courts in the enforcement of international human rights, especially the human rights cases that came down in the 1980s and were known as "Filartiga's progeny." As an international human rights lawyer I found both volumes of interest, whereas I wonder if they would have the same attraction for a nonspecialist. Barbara Yarnold's *International Fugitives*, on the other hand, deals with the failure of international extradition procedures.

Yarnold's thesis is that contemporary international extradition procedures are not working and that their violation endangers international peace and security.

This is a debatable argument. Yarnold examines two case studies. The first involves the illegal kidnapping on 11 May 1960 of Adolf Eichmann in Argentina by Israeli agents in order to try him in Israel on charges of responsibility for the commission of crimes against humanity. The second involves the seizure of Manuel Noriega, who surrendered to U.S. authorities on 3 January 1990, following the United States invasion of Panama, in order to try him in the United States on drug trafficking charges. Did either of these incidents lead to the endangering of world peace and security?

In her study of the Eichmann case, Yarnold points out that the Israeli government, in order to preserve the long-standing friendship between Israel and Argentina, a month after the abduction of Eichmann, forwarded to the Argentine government a lengthy document explaining away certain issues in the case. Yarnold states that two deliberate misstatements were made in this letter: (1) that Eichmann was abducted by private volunteer groups when, in fact, these groups were supported by the Israeli government and (2) that Eichmann went to Israel of his own free will when, in fact, Eichmann was picked up by Israeli agents on a public street, taken to a private home, and then placed on a plane bound for Israel without his consent. The Argentine government responded two days later with a letter of protest claiming that its sovereignty had been violated and demanded the return of Eichmann. In the face of Israeli refusal to return him to Argentina and to risk the possibility that Argentina might choose not to extradite him, on 15 June 1960 the Argentine government filed a formal complaint with the Security Council of the United Nations, charging that the Israeli action "created an atmosphere of insecurity and mistrust, incompatible with the preservation of international law."

On 23 June 1960 when the UN Security Council considered the matter, the vote was eight to zero in favor of Argentina. Although the resolution requested Israel "to make appropriate reparation," Yarnold informs us that no such reparation was ever made and that Eichmann remained in Israel. Eichmann was tried and convicted in Israel. He was sentenced to death on 15 December 1961 and hanged on 31 May 1962. If this case teaches us anything, it is the inefficacy of UN Security Council resolutions. The government of Israel got its man, did what it wanted with him, and suffered, at most, a slap on the wrist.

Even recognizing that the United States invasion of Panama, undertaken (in part) to seize Manuel Noriega resulted in the loss of lives, can it be said that this invasion endangered world peace and security? I think not.

To remedy this state of affairs, however, Yarnold advocates that jurisdiction of international crimes should be transferred to the International Court of Justice. In addition, she suggests, jurisdiction over state crimes should be transferred to the International Court of Justice when fugitives have crossed national boundaries and are outside the state in which they committed their crimes. At present, it appears unlikely that states will cede the sovereignty requisite to allowing the International Court of Justice to decide when extradition is appropriate. Some of Yarnold's suggestions, such as the creation of an international prison "so as to avoid imprisonment in states that might transgress the international and human rights of criminals" appear a little whimsical—as, indeed, is her optimism regarding "the contemporary disintegration of the superpowers [which] may facilitate the adoption of these recommendations by the world community." For the record, one superpower claims not yet to have disintegrated.

On a more substantive note, however, it is regrettable that Yarnold did not look to international human rights tribunals for the precedents that they are already establishing in the international adjudication of extradition requests. Specifically, the European Court of Human Rights handed down a unanimous judgment in 1989 in the *Soering* versus *United Kingdom* case, in which the Court held that if the individual in question were to be extradited to the United States, the prohibition under Article 3 of the European Convention, dealing with "inhuman or degrading treatment" would be violated. Soering, a German national wanted for the capital murder of his girlfriend's parents in the state of Virginia, fled to Britain, where he was subsequently arrested. Under the terms of the 1972 United States–United Kingdom Extradition Treaty, Britain could refuse to extradite him if the offense for which the extradition was requested was punishable by death and the requesting party failed to give assurances that the death penalty would not be carried out. Under British law, Soering was found to be psychologically impaired and could not have been found guilty of murder. Soering petitioned the European Commission of Human Rights; but since the European Convention on Human Rights allows for the imposition of the death penalty, Soering based his complaint on the prohibition of "inhuman or degrading treatment or punishment," which he claimed would ensue from the "death row phenomenon" involved in the six-to-eight-year expected delay in carrying out the death sentence in the state of Virginia. This decision of the European Court of Human Rights persuaded the United States to give appropriate assurances to the British Government that no capital murder charges would be brought against Soering, who consequently was extradited, tried and convicted, and sentenced to two life terms, having successfully avoided the death penalty.

Kenneth C. Randall's *Federal Courts and the International Human Rights Paradigm* provides a wealth of knowledge on the role of the federal judiciary and related issues, such as the relationship between federal and state courts and the relationship of the judiciary to the other branches of government; but his thesis is that federal courts should assume jurisdiction over not only human rights cases but also cases involving acts of terrorism, since the victims of one are as innocent as the victims of the other and the perpetrators equally reprehensible.

Notwithstanding the moral force of this argument, Randall misreads human rights law. He argues that the UN Covenant on Civil and Political Rights "bars sovereign *and nonstate* actors alike from committing torture." In fact the UN Covenant and all other international human rights treaties only bar state actors or persons acting under the color of law. The UN Convention against Torture and Other Cruel, Inhuman, or Degrading Treatment or Punishment explicitly defines torture as pain or suffering "inflicted by or at the instigation of or with the consent or acquiescence of a public official or other person acting in an official capacity"; but the color-of-law requirement is implicit in all other treaties. Since the prerequisite for presenting a case before an international tribunal is that the violation is alleged to have been committed by a person acting under color of law, petitioners are barred from bringing cases under the international human rights treaties against private parties.

World Justice? is an excellent compilation of articles written by international lawyers and political scientists on the subject of the enforcement of international human rights norms. I found this to be the most informative of the three books under review. Dan Bodansky's lead essay, "Human Rights and Universal Jurisdiction," starts from the premise that one cannot rely on domestic court implementation of human rights norms, since often, the government that would hear the claim is the one that is committing the violations. Consequently, human rights activists have begun to use the judicial system of one country to redress violations in another, such, as in the *Filartiga* decision. He notes, however, that the Alien Tort Claims Act has been the source of controversy. Does it create a federal cause of action for violations of international law or only confer jurisdiction on federal courts in cases where the cause of action already exists under international or domestic law?

Bodansky converts a difficult topic into lucid and elegant prose, simplifying the issue for the nonlawyer or the lawyer not familiar with this area of the law. He invokes the international law concept of universal jurisdiction to provide the legislation that would permit suits in federal courts for human rights violations regardless of where they occurred. But in order for universal jurisdiction to be exercised, he explains, the court must be ready to apply international law. A number of treaties exist that have created international crimes and have established both individual and state responsibility, as is the case with the treaties criminalizing genocide, apartheid, and torture. Bodansky distinguishes these new international norms of individual responsibility from individual responsibility for earlier offenses such as piracy or slave trading in that these newer norms can only be violated by individuals acting in an official, not in a private, capacity. Human rights offenses, such as torture, he points out, can only be committed by persons acting under color of law. This rule, which is the cornerstone of all international human rights law, is today gradually being eroded as some states, while others seek to press human rights organizations to expand their mandates to investigate human rights violations, (e.g., killings) perpetrated by such private parties as guerrilla or terrorist organizations.

In chapter 2 of *World Justice?* Steinhardt examines the "doctrines of judicial diffidence." The doctrines of foreign sovereign immunity, the act of state doctrine, and the political question doctrine are treated as representing the unwillingness of the judicial branch to compete with the other branches of government in the area of foreign affairs. The conviction that the state must always speak with *one voice* means viewing international law (according to Steinhardt) as a kind of "ritualized politics" and not as law at all. He recommends that this one-voice orthodoxy be dispensed with in cases with an international human rights component.

John Quigley, in "Enforcement of Human Rights in U.S. Courts," submits that the forcible abduction abroad of persons for the purpose of bringing them to trial in the United States (e.g., Noriega) violates international human rights law. He argues that U.S. courts should refuse to accept jurisdiction over a criminal defendant who has been illegally brought into the United States.

Editor Mark Gibney, in "Courts as 'Teachers in a Vital National Seminar' on Human Rights" suggests that the courts have a didactic role to play in society. Concerned about the incongruity in the United States' treatment of its own citizens as compared with its treatment of citizens of other countries (e.g., Panama, Nicaragua, and El Salvador), Gibney criticizes the failure of the courts to play a role in educating the U.S. population in international affairs. He points out, for example, that in 1987 when the Iran-Contra hearings began, over half of the American public did not know who the Contras were, or which side the United States was supporting. Consequently, the much vaunted support of the Contras by the U.S. public following the congressional testimony of Colonel Oliver North is not to be given much importance, since such support is ephemeral. Litigation, Gibney convincingly argues, would educate the American public because the population would be confronted in a very public way with the effects of U.S. policies: "U.S. policy needs to be publicly defended, [and] the human consequences must be made known and compensation paid to innocent civilians who suffer from the pursuit of these policies."

John Rogers, writing on international human rights law and U.S. law, notes that most international human rights law is not applicable law in U.S. courts because the United States has not ratified the major international human rights instruments. United States law does, of course, afford extensive protection against human rights abuses, he explains, by means of the Bill of Rights and the post–Civil War Amendments to the Constitution. In addition, he adds, other rights are protected by federal and state law.

Rogers argues the other side of the argument presented by Steinhardt and Gibney that the courts should defer from deciding issues that involve the foreign relations of the United States in the interest of preserving the one-voice doctrine of the conduct of international affairs. Rogers maintains that the courts should not determine whether the actions of the U.S. government are legal or illegal.

Howard Tolley, Jr., in a very informative article on the role of human rights interest group litigation in persuading U.S. courts to apply international human rights norms, describes the different groups that have litigated human rights claims in federal courts. He reveals the little-known fact that it was a trademark lawyer named Peter Weiss, of the Center for Constitutional Rights, who unearthed the obscure Alien Tort Claims Act of 1789 and converted it into a viable vehicle for the presentation of human rights claims. He also points out that many of the "celebrated legal successes" in the 1980s were Pyrrhic victories, given that the courts handed down generous awards (e.g., the $10 million awarded in the *Filartiga* case) but that none of the defendants ever paid. In conclusion, Tolley notes that political factors may ultimately matter more than legal doctrines—"as the NAACP discovered in its eighty year quest for racial justice."

Bert Lockwood in the very creative "Toward the Economic *Brown*" predicts that within the next 35 years the U.S. Supreme Court will come down with an "economic rights" case of the impact of the *Brown* decision on basic needs or subsistence rights. Lockwood has argued elsewhere that international human rights law played an uncredited role in setting the stage for the Supreme Court's attack on American segregation that led to the 1954 *Brown* decision. Similarly, he expects international human rights law in the economic and social area to lead to a *Brown*-like decision in the area of economic rights. His candidate for the constitutional "window" that the Supreme Court could be expected to invoke "for the insinuation of international economic norms" is the privileges and immunities clause of the Fourteenth Amendment of the Constitution. Lockwood's article, in my view, is the most original and creative piece in this volume. Since my own research has taken me into examining the intent and possibilities inherent in the privileges-and-immunities clause in regard to protecting fundamental human rights, I found this argument particularly attractive.

The final article, by Anthony D'Amato, discusses the importance of the interests of the individual when compared with the historical primacy of the state in international law. D'Amato reviews the case of his client (*Frolova*), in which an American woman sought the release of her Russian husband from the Soviet Union. Human rights law, he suggests, has enabled international morality to catch up with international law.

Inter-American Commission on Human Rights Organization of American States CHRISTINA M. CERNA

Soviet Policies in the Middle East: From World War II to Gorbachev By Galia Golan. New York: Cambridge University Press, 1990. 319p. $44.50 cloth, $14.95 paper.

In the period prior to the collapse of the Soviet Union and the resignation of its leader, Mikhail Gorbachev, Western observers of Soviet policy toward the Middle East were divided into three major groups. The first, represented by the author of this volume, Galia Golan, has asserted that Soviet policy towards the Middle East since World War II has essentially been defensive in nature. The second (which I endorse) has argued that Soviet policy toward the region, at least since the mid-1960s, has been offensive in nature, aimed at eliminating Western (particularly American) influence from the region—unsuccessfully, because of the independence of the states in the region and their ability to manipulate the Soviet Union. Finally, a third group of analysts argued (at least until 1989) not only that the Soviet Union's policy toward the Middle East was offensive in nature but that it had been successful in spreading its influence over the region.

Golan's book analyzing Soviet policy toward the Middle East since World War II closely follows the "defensive" approach reflected in her earlier writings. It is a clearly written, well-argued overview of Soviet policy that despite its lack of notes, would be a useful undergraduate text in Soviet foreign policy or Middle East politics courses. Golan compensates for the lack of notes by a "Guide to Further Reading" for each chapter, as well as a detailed bibliography.

The book is roughly divided between Soviet policy toward the Arab–Israeli conflict (Soviet policy toward Egypt, Syria, the Six-Day War, the 1967–73 interwar period, the Yom Kippur war, the Arab–Israeli conflict, the 1982 Lebanese war, and the Palestinians and the PLO) and the Persian Gulf (Iraq, Iran, Islam, South Yemen, and the Arabian peninsula). It also has a concluding chapter on Gorbachev's Middle East policy current through early 1990.

Throughout the book, there is a clear emphasis on the Soviet Union's need for military bases and facilities as a major motivating factor in its Middle East policy. Thus, the Soviet quest for influence in Egypt in the mid-1960s was due in part to a need for bases for the Soviet Mediterranean fleet, which was deployed against U.S. Polaris missile–carrying submarines. Similarly, Moscow's shift of interest to the Persian Gulf in the 1970s is seen as closely linked to the U.S. deployment of Poseidon and Trident missle-firing submarines in the Indian Ocean. Golan also notes the basic caution with which Moscow approached the Arab–Israeli wars of 1956, 1967, and 1973, asserting, quite correctly, that Soviet threats in all three wars were issued "after the major fighting on the ground was virtually finished and the actual need to implement the threat was most unlikely" (p. 92).

If one starts from a different perspective, however, a number of the author's assertions about Soviet policy become open to question. Thus, Soviet assistance to the Syrian army in its move into Jordan in September 1970 can be seen as a low-risk attempt to overthrow King Hussein of Jordan, then a close ally of the United States. Similarly, the Soviet Union could have profited from its client South Yemen's invasion of North Yemen in 1979, given North Yemen's alignment with the United States at the time. While both of these cases are somewhat ambiguous, Moscow's complicity in violating the July 1970 Egyptian–Israeli ceasefire by sneaking antiaircraft missles up to the canal (an incident Golan skips over) was not. Nor can Soviet military assistance to the Arabs during the Yom Kippur war or its applauding of the Arab oil embargo against the United States be called defensive actions.

One might also raise a few questions about Golan's interpretation of several Middle Eastern events. Thus, Camp David, rather than the Sinai II agreement, should be seen as the "nadir of Soviet–American relations in the Middle East" (p. 101). Similarly, to assert that the United States refused the Soviets even token participation in the Arab–Israeli peace process from 1975 onward (p. 103) overlooks the Soviet–American joint statement of 1 October 1977 in which the two superpowers agreed to reconvene the Geneva Conference by December 1977. Although the joint statement became a dead letter because of Sadat's visit to Jerusalem in November 1977, the fact remains that from July to October 1977 the Carter administration was moving to allow the Soviet Union a role in the peace process; and the 1 October 1977 joint statement was evidence of a change (albeit only a temporary one) in U.S. policy. Finally, one could question Golan's assertion that Israel's invasion of Lebanon in 1982 had "discredited Arafat's diplomatic–political tactics, strengthening the radical, rejectionist forces in the organization (p. 134). In fact, when no Arab state aided the PLO during its seige in Beirut (Syria had agreed to a ceasefire with Israel while the PLO was still fighting) and Arafat was kicked out of Lebanon a second time (in 1983, by the Syrians), he journeyed to Egypt and began a political reconciliation with Egypt and Jordan that led first to the Hussein–Arafat agreement of 1985 and finally to the PLO's agreement to a two-state solution to the Arab–Israeli agreement and recognition of Israel's right to exist in 1988. While Golan argues that Moscow tried to dissuade the PLO leadership from formally declaring itself in favor of the two-state solution (p. 277), in fact, Moscow had long seen a two-state solution as the optimum one for the Palestinian–Israeli component of the Arab-Israeli conflict; and it had repeatedly urged the PLO leadership to adopt such a position—particularly under Gorbachev, who wanted to eliminate Third World areas of conflict that might endanger U.S.–Soviet relations.

Given the collapse of the Soviet Union and the emergence of the Central Asian Muslim republics as quasi-independent states of the new commonwealth, Golan's analysis of the situation (p. 208) in these republics is of great significance, in light of fears in some Western circles that Islamic fundamentalism might sweep the region: "The Soviet Muslim problem was an ethnic one, especially among Muslims of Turkic origin (who constitute the largest group of Soviet Muslims). . . . This was not a matter of religious revival or fundamentalism, in which Soviet Muslims showed little interest. It was, rather, a matter of national sentiments of which Islam is an historically essential component" (p. 208).

In sum, despite some difference in interpretation, I think Golan's book is a most worthwhile contribution to the literature on Soviet policy in the Middle East.

Baltimore Hebrew University ROBERT O. FREEDMAN

Foreign Policy and Ethnic Interest Groups: American and Canadian Jews Lobby for Israel. By David Howard Goldberg. New York: Greenwood, 1990. 197p. $45.00.
The $36 Billion Bargain: Strategy and Politics in U.S. Assistance to Israel. By A. F. K. Organski. New York: Columbia University Press, 1990. 315p. $30.00.

It has been frequently stated that the influence of the pro-Israel lobby groups on U.S. policy toward Israel and the Middle East, especially the American Israel Public Affairs Committee (AIPAC), has often been so great as to have virtually controlled American policy in this region. However, although the two books reviewed here by Goldberg and Organski utilize different research designs and data, their conclusions are roughly similar in showing that what has passed for common knowledge about this subject has been based on a misreading of the facts and that the AIPAC's influence has never been that great.

Goldberg's study systematically compares two organizations that have attempted to influence governmental policy toward Israel, AIPAC in the United States and the Canada–Israel Committee (CIC) in Canada. He examines the AIPAC's behavior during eight situations (the Yom Kippur War, the Kissinger diplomatic offensive, Carter's diplomatic strategy, antiboycott legislation, the F-15 controversy, the Airborne Warning and Command System sale, Lebanon and the Reagan Plan, and the Palestinian disturbances) and the activities of the CIC in seven (the Yom Kippur War, the PLO in Canada, antiboycott legislation, the initiation of the Jerusalem pledge, the reversal of the embassy imitiative, the Lebanon War, and the Palestinian disturbances) (pp. 6–7). Then, each group's behavior is compared in terms of six factors that assist in explaining to what extent the lobby groups have had an impact on governmental policy: the group's behavioral attributes and organizational characteristics, the structure of the state's foreign policy decision making, the domestic and international political environments, and the policy issue itself. Finally, Goldberg utilizes a crude but servicable scale in order to rate the influence of each lobby group during every situation.

What does the author find? Four of his conclusions will be noted here. First, neither the AIPAC nor the CIC were as influential as often has been stated, but the AIPAC was *more* influential. Second, in order to be influential, an ethnic interest group must organize itself to fit the constraints of the governmental system in which it operates. Third, the American system is more open to ethnic group involvement in foreign policy issues than is Canada's. Finally, the extent to which a national government, especially its top executive, cares deeply about an issue and is determined to see a chosen policy put into effect is critical.

Organski's *$36 Billion Bargain* examines the influence of the pro-Israel lobby (including the AIPAC) on the granting of American aid to Israel. His first part covers four major elements of the American domestic scene: presidential support for aid to Israel, American attitudes regarding Israel, the pro-Israel lobby's effect on the behavior of Congress, and the fit of U.S. policy toward Israel (including aid) to the American government's Middle Eastern policy and overall competition with the Soviet Union.

The second part of Organski's study looks at a number of aspects of international politics that have affected America's assistance to Israel. This portion of the book contains a lengthy discussion of the meaning of the term *assistance*. It also presents data comparing U.S. assistance for Israel to U.S. aid given other countries in the world as well as comparing all forms of U.S. assistance to Israel with same types of assistance for the other countries in the Middle East. It challenges many of the oft-heard statements about one-sided U.S. assistance for Israel.

Organski's findings are interesting and important. For example, he shows that U.S. governments, especially since 1973, have viewed assistance to Israel as clearly being in the interests of American foreign policy in toto and in the Middle East. The author also shows that although the AIPAC is a capable organization, its requests have merely tended to parallel the preferences of U.S. presidents, not determined them. When a president strongly wishes to go against the AIPAC's desired policy, he is readily able to do this and succeed. Furthermore, the author convincingly shows that American assistance to Israel has been an excellent bargain for the United States in that it has been moderate in its extent, well utilized by Israel, and helpful in achieving a number of U.S. foreign policy goals. In few other places, states Organski, has American assistance been better spent.

These excellent studies' widely differing approaches and types of evidence produce fairly similar conclusions about the influence of the pro-Israel lobby in the United States. The pro-Israel lobby, whatever its skills, organizational strength, and finances, succeeds only when its preferred policies also are viewed as in America's interests by many other elements of the U.S. political system. But it furthermore appears to have been in the self-interest of all of the groups involved in making or attempting to influence U.S. foreign policy in the Middle East to create and defend the false image of an exceptionally powerful pro-Israel lobby. Despite the high quality of these books, it is unlikely that they will effectively challenge existing common knowledge regarding the AIPAC's great influence. Nevertheless, these books should be required reading for anyone interested in the effects of ethnic group influence and especially that of the AIPAC on U.S. foreign policy.

University of Virginia JOSEPH M. SCOLNICK, JR.

Cooperation among Nations: Europe, America, and Non-Tariff Barriers to Trade. By Joseph M. Grieco. Ithaca: Cornell University Press, 1990. 255p. $39.95 cloth, $13.95 paper.

Joseph Grieco abbreviates, synthesizes, and (perhaps) oversimplifies current writing on international politics into two categories identified as *realist* and *neoliberal institutionalist*. He examines how the two schools approach problems of international cooperation, given their shared assumption of anarchy as characterizing relations among states. His analysis finds neoliberals relatively optimistic about prospects of institutional cooperation and mainly concerned with the issue of cheating on agreements but believing that such issues can be solved by arrangements agreed upon by states acting as *rational egoists* (*independent maximizers* to economists).

Realists are described as pessimistic about the prospects of cooperation with regard to both the issues of cheating and concerns (generated by their image of anarchy) that some partner in a relationship may gain greater advantages than others. In this realist analysis, to fall behind some possible future adversary, even while gaining benefits from cooperation, is to risk future domination. Hence, states acting on realistic assumptions are expected to act as *defensive positionalists*, reluctant to enter or honor cooperative arrangements.

The author subjects these two approaches to a logical analysis that concludes that realism is more consistent with the condition of international anarchy as here defined. He takes for granted agreement by both schools that realism both dictates and describes strategies with regard to national defense (p. 12). Then, in the more substantive sections of the work, Grieco examines data on United States–European Community interactions in relation to *six codes* designed to reduce *nontariff barriers* to trade negotiated in the Tokyo Round of (GATT) deliberations. That wealth of carefully explored and outlined data is used to test hypotheses drawn from the two theories. The use of these data on the actions of allied, developed democracies regarding economic arrangements (low politics), rather than data on strategic issues of national security (high politics), is asserted to bias the case in favor of the neoliberal approach.

The conclusion of this empirical test is that neoliberal and realist approaches are equally able to explain positions taken on the four more effective codes but that realism offers a more adequate explanation of actions on the two less effective codes (technical standards–government procurement policies and subsidies in relation to countervailing measures). The topics on which realist theory has the advantage involve strategic issues that were to be excluded from the test (p. 182).

Indeed, nontariff barrier issues generally cut across the artificial bounds of high and low politics. Nontariff barrier issues related to food self-sufficiency and the geographic distribution of industrial capacity still have important security dimensions for nations so recently at peace. If the author were really looking for "cases involving trade (or other economic) relations among the advanced democratic states" to test the theory (p. 14), then something like the United States–Canada trade agreement would have been more appropriate.

The author finesses the apples–oranges problem of examining United States–European Community interactions by asserting that positions taken by the latter coalition were those of its dominant partners. The issue might have been resolved as easily by assuming that the actors were agents of highly pluralistic societies. The author notes that well after the codes have been operating, their economic consequences remain undocumented and "difficult to estimate and, thus, to compare" (p. 158). If that remains the case, it would seem unduly difficult for agents to have anticipated the consequences for their national power positions that realist theory is said to require. By contrast, however, the data discussed make it clear that consequences of a code were obvious for particular economic interests (winegrowers or pastamakers) in the nations involved. The emerging branch of political economy literature on endogenous protection, such as Magee, Brock, and Young's *Black Hole Tariffs and Endogenous Policy Theory* (1989) strongly calls into question the validity of this realist dichotomy between international and domestic politics if not the entire concept of the unitary state.

Further, there is are implicit presumptions that each accord or potential accord within GATT would necessarily induce freer trade and that any agreement that would promote freer trade would necessarily generate net economic benefits for all GATT signators. Neither of these assumptions is universally accepted. Protection is fungible. Giving up degrees of freedom in areas not extensively used (or exchanging one type of protection for a more politically expedient one) is simple and may look like progress toward freer trade and still lack quantitative impact.

One does not have to be a proponent of strategic trade in the Krugman sense to reject the second assumption. Twenty years ago, a top mainstream trade economist, Harry Johnson, stated, "The case for free trade, frequently asserted with considerable dogmatism in the past, appears in contemporary international trade theory as an extremely qualified proposition, dependent on the maintenance of international monetary stability, on efficient representation of alternative social opportunity costs by money costs and prices in the domestic currency, on the social acceptability of the resulting distribution of income or the adoption of a social policy with regard to income distribution and on the possible need for international income transfers" (quoted in *Journal of Economic Literature* 8(1970): 438–39). Even if there is still agreement on the benefits of free trade, the theory-of-the-second-best states that a partial move toward free trade from a distorted starting point may not be welfare-improving.

Grieco's study concludes with several well-conceived hypotheses for further inquiry but without indicating sets of events in which they can be tested. Given that Europe has long been a cockpit for warring nations and an inspiration for realist theory, it seems relevant to explore the emergence of a European Community able to function as an agent in the negotiations discussed. The problem for realist theory as described here would seem to be to explain not the resistance and obstruction (emphasized in defense of the theory) but such cooperation as did occur.

I would suggest two further events to analyze in light of the competing theoretical perspectives: the emergence and strengthening of other regional groups and the implications for cooperation among nations in a world increasingly characterized by a concentration of military power and a diffusion of economic power. The United States is willing to enter into a trade agreement with Mexico that is likely to produce larger gains for Mexico at a potential cost for some U.S. social groups in the short run. The reasons relate not just to multinational corporations and cross-border alliances but also to the perceived cost of the alternatives. The United States sees increased access to lower-cost labor as being the only way to avoid further losses in manufacturing competitiveness to Asia and the enlarged European Community. Such forces for cooperation are ruled out in the simple two-country game-theoretic frameworks that underpin both schools. On the second point, realist theory implies that concern with relative gains is strongest among states of moderate power and weakest for either all-powerful or impotent states. Thus, we can imply that the prospects for military collaboration (the actions against Iraq) are improving with the end of the cold war but that prospects for economic cooperation are deteriorating (as typified by the GATT stalemate) in a multipolar economic world.

Grieco has a gift for simplifying and categorizing complex issues. In many instances, useful teaching and learning tools emerge, as well as some testable implications for analysis. Yet the reader must realize that much of the original flavor and some caveats of the original works may be lost in the process. His need to see things in black and white, abstracting from situations and eras, leads to claims more strident than any others in the current debate. For example, "This book attempts to demonstrate that realism is logically superior to neoliberal institutionalism." Again, "If neoliberalism has difficulties in accounting for U.S.–EC code interactions, . . . its shortcomings as an analytical approach will have been highlighted. . . . Hence the [nontariff barrier] code experience may approach the status of a crucial experiment" (pp. 11, 14). Contrast this with a statement like the following, from Gilpin's *Political Economy of International Relations*: "The clash between liberals and nationalists over the benefits and costs of specialization, although partially based on differing economic and political objectives, also rests on differing assumptions regarding the nature of international economic relations. Liberals consider these relations to be essentially harmonious; nationalists believe that conflict is inevitable. . . . Neither assumption is valid in itself. Rather, its validity rests on the larger configuration of global economic and political conditions at a particular time" (1987, p. 188). It is encouraging to see that some realist scholars see the world as large enough to hold a multiplicity of ideas and too round to be fully comprehended from a single point of view.

Institute for Economic Development and Policy — ROBERT K. MCCLEERY

Artificial Intelligence and International Politics. Edited by Valerie M. Hudson. Boulder: Westview, 1991. 422p. $55.00.

This anthology is a valiant effort to make sense of a yet immature branch of political science. For this reason, the book is well worth reading. It provides little in the way of research findings, nor is it a coherent collection of essays. But it is glimpse into a discipline undergoing change, a tribute to the interdisciplinary nature of political research, and a case study in its own right about the emergence of new tools and new perspectives for the scientific study of things political.

I will focus on four questions: Why is the application of artificial intelligence to politics an immature science? What does it offer to the study of the political human being? What are its basic tools? And why is so much artificial intelligence work focused on international politics?

Most of the essays collected in this volume draw upon the vocabulary of artificial intelligence, and most are examples of computational modeling. These efforts are collectively immature in the sense that those using the vocabulary and tools of artificial intelligence arguably know more—but do only marginally better—than those without that vocabulary and set of tools. Future work will certainly change this calculus for the better.

There are other signs of the field's immaturity. The articles present several implicit or explicit typologies for categorizing differences within the computational modeling community, yet none seems to capture or bring together the diversity found in these pages. Perhaps the most useful distinction made in these pages is also the simplest, the oldest, and the least specific to artificial intelligence modeling, namely, the difference between static and dynamic models—the latter having some capacity for in-process "adaptation" and "learning."

The most telltale sign of the field's immaturity (as reflected in the book) is the authors' apparent need to recommend a domain of inquiry, a method of inquiry, and findings from that inquiry. Many, although not all, of these articles seek to recast the political human being as an information processor. Many simultaneously seek to demonstrate how computers and higher-level structured languages can be used to model their revised political human being. Some articles even conclude with a set of findings that are to add to the discipline's cumulated wisdom. This triple exercise would be roughly akin to a behavioralist simultaneously recommending political psychology, explaining the use of SPSS or SAS in social research, and then listing what others should treat as revealed truth. It is an immature science whose domain of inquiry and tools of inquiry are at least as problematic as its sparse findings.

What about its domain of inquiry? Three recurring themes about the political human being as an information processor emerge: the human being is a good information storage system, at best a modest information retrieval system, and a poor calculator. The latter, of course, pits these computational modelers against their rational choice cousins.

What follows from these fundamental themes is more questionable. If the end of political science is to understand the political human being's limitations, then attention to the oft-mentioned standards of process validity (looks like the real thing) are as important as replicating the political choices flowing from those processes. But if the end of political science is to help the political human being transcend human limitations, then replicating human folly is important only as a starting point for its improvement. There are other forms of myopia in these pages. In an article with admittedly no focus on process validity, Schrodt argues that his project "seeks to apply pattern recognition in a world of behaviors produced by pattern recognizers" (p. 187). Surely, we can all identify a few pattern recognizers who, rather than being condemned to repeating event sequences, seek to change those sequences. The article does, in any event, review an algorithm for processing event sequences in order to discriminate between war and nonwar crises.

What are the basic tools of artificial intelligence? It is not the use of computers, per se. Nor is it the use of high-level structured languages such as LISP, which in turn might be written in decidedly low-level languages like C and Assembler. Together, computers and appropriate programming languages do permit the researcher to create a technological instance (instantiation, if you will) of various images of human information processers and the work that they do. As noted, these same tools can also be used to add strengths where humans fall short, particularly in strategic optimization.

The volume contains an example of an extraordinary use of computers and computational languages. Mallery's article on semantic content analysis is as interesting as his reliance on jargon is unfortunate. Beneath discussions of eidetic representation, lexicalist semantics, sentential constraints, and so on lies an imaginative computer program for processing text files into semantic structures and passing these through classification systems such as Lasswell and Namenwirth's value dictionary. The program has a growing library of classification schemes; but these classification schemes are provided to, and not produced by, the program. Still, a considerable amount of work remains before RELATUS even comes close to realizing its "creativity-respecting foundations for the textually oriented social sciences of the future" (p. 119).

Finally, why has so much artificial intelligence and computational modeling so far been concentrated in the field of international politics? Not by accident did the editor choose to focus on that subfield, for remarkably little artificial intelligence work exits outside of it. In part, this concentration flows from the subject matter of international politics and its previous methodological traditions. Quantitative international politics has long included dynamic mathematical modeling, and the art of modeling forms a common bridge between the older mathematical, and the newer computational, endeavors. The subject matter of international politics also lends itself more immediately to this nascent science. Plato sought to understand the just individual by looking at the just state; and the study of the political human being as an information processor is easier in the "reconstructed logics" of foreign policy than in the average human being's "logic-in-use."

All told, artificial intelligence in the study of international politics is not a fad. As the paradigm takes shape, its conceptual and methodological moorings will become less problematic, and the work it can do will become more compelling.

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STEVEN THOMAS SEITZ

Paradoxes of War: On the Art of National Self-Entrapment. By Zeev Maoz. Boston: Unwin Hyman, 1990. 368p. $39.95.

This book approaches the causes, management, and implications of international war as a series of related paradoxes. To set the stage for his approach, Maoz convincingly argues that war is usually treated with linear logic that, while useful, cannot accommodate many nonlinear aspects of the conflict process. The purpose of his work is to explore the "paradoxical" nature of war. When conflict is viewed as a set of unraveling paradoxes, Maoz contends, many nonobvious implications hidden in the record come forth with clarity. Maoz's theoretical objective is to replicate the process used by Kenneth Arrow to unravel the paradox of voting proposed by Condorcet, which dictated the research agenda in the field of voting and helped uncover major insights never before suspected.

Maoz develops several theoretical handles to generate war paradoxes and to account for misunderstood loops that emerge before, during, or after war. The analysis of contradictions found in wars are derived using rational and cognitive perspectives. Maoz argues, with some validity, that the merger of rational and cognitive approaches provide competing yet complementary perspectives that, combined, can account for the wages of war. From a rational perspective, we are told, paradoxes occur because actors do not control the rules of the game or the choices of other actors. From the cognitive perspective, paradoxes arise because imperfect understanding of conditions lead actors to behave suboptimally. While such differences may be real, one would have liked to see (at least for each paradox) an integration of perspectives. For example, the cognitive logic that guides the behavior of an individual descending a tree who anticipates that branches in the next sequence may break, can also be effectively represented by sequential game theory (as shown by the recent work of Robert Powell, Bruce Bueno de Mesquita and David Lalman). To provide a coherent theoretical perspective, it would be useful to distinguish the case in which the two approaches produce similar results from that in which they differ substantively so that much can be learned by employing both. Maoz does not provide such an integration. Instead, both perspectives are elaborated side by side throughout the book, leaving the reader to decide when results are contradictory, mutually exclusive, or complementary. This detracts somewhat from the otherwise very clear presentation of paradoxes on war, since some confusion is generated when very complex behavior is given a dual explanation.

Maoz argues that paradoxes do not result from deliberate behavior but emerge as the unexpected, unintended consequence of actions that lead to paradoxical loops and force elites to adopt ever more undesirable positions in conflict. Consider how such paradoxes and their sequencing affect the study of war. Maoz shows that actors with superior capabilities do not always emerge victorious in the power paradox. He draws on quantitative and nonquantitative research alike to demonstrate the lack of consistent explanation for the failure of the United States to win in Korea when it held a near monopoly of nuclear power, as well as the failures of the United States in Vietnam, the Soviet Union in Afghanistan, China in Vietnam, and Israel in Lebanon to bring a successful conclusion to each conflict when each held nuclear preponderance over opponents (p. 232). There is a link among paradoxes. When the paradox of power applies, one frequently encounters the sunk cost paradox that delayed American exit from Vietnam and Israel's withdrawal from Lebanon long after the original goals in such war could not be achieved (p. 283).

Moreover, the loser's paradox reinforces the effects of the prior two. Maoz shows that "Phoenix factor" effects after major wars reemerges in the Middle East where Jordan (the loser) benefits substantially from defeat and partition while Israel (the winner) does not. Maoz informally relates paradoxes to each other and to linear findings picturing the process of war but does not elaborate a theoretical framework to account for these sequences.

After reading a book so full of insights, one seeks an overarching structure to simplify the study of war. Such a structure is not forthcoming, yet the foundations for a nonlinear analysis of war are clearly delineated. Maoz contributes a new, systematic perspective on war that suggests much promise. This is a solid contribution incorporating new theoretical insights in a systematic attempt to answer the key unobvious questions surrounding the paradoxical process that leads to the waging of wars. The drawback is that having persuaded the reader that paradoxes do exist, Maoz does not provide a theoretical or practical handle to identify when such paradoxes will emerge, what consequences they will have, how much will they distort expected results, and (most importantly) how policymakers can handle these unexpected departures that lead to consequences undesired by all.

Maoz must be commended for opening an important, seldom-traveled avenue with enormous promise for understanding the process of war. On balance, Maoz has made a significant contribution to the study of war. This clearly written, concisely argued, and effectively documented record of unusual paradoxes associated with the waging of war opens new avenues of inquiry and adds sensitivity to the treatment of this complex process. The linkages between and among paradoxes and linear explanations are informal, leaving much to be explored. Analysts interested in international war will find in this book a rich vein of insights whose interconnections will no doubt be explored for some time to come by graduate students concerned with war. For these reasons, I recommended this volume for middle-level courses on conflict and consider the book required reading for graduate courses on conflict.

Vanderbilt University JACEK KUGLER

As Moscow Sees Us: American Politics and Society in the Soviet Mindset. By Richard M. Mills. New York: Oxford University Press, 1990. 308p. $29.95.

The publication of the Mills's book at this time gives rise to a fundamental question. In the face of the intellectual revolution unleashed by Gorbachev, what do Soviet Amerikanisty (Americanologists)—whether hard-nosed conservative or emancipated liberal—reveal about the way "Moscow sees us"? Mills spells out the main features of what he refers to as the Soviet intellectual *mindset* regarding American politics and society. Soviet Amerikanisty, he argues, remain wedded to a basically Marxist view of the United States. He starts with a discussion of "state monopoly capitalism," examining the relationship between economic classes and political institutions, that is, how the distinctive relations between the "rulers and the ruled" affect U.S. politics, the presidency, elections, political parties, and the Congress. With increased intellectual and political contact with the outside world and greater familiarity with American political science, he correctly argues, Soviet analyses have become analytically more sophisticated. Thus, unlike traditionally oriented Soviet writers, current observers are willing to examine previously unintelligible features of U.S. political life, such as the weakness of the American Left and the virtual irrelevance of the Communist Party of the United States of America (CPUSA).

Accepting as a given his analysis of the increased Soviet understanding of the American political scene, Mills is most arresting when he observes, "No substantial changes have taken place in the way the Soviets view American domestic society." The persistence of these views is puzzling. Since the mid-1980s, the class analysis discussed by Mills has come under serious attack in Soviet academic circles. The repudiation of Marxism by the reformist intelligentsia has led to the wholesale rejection of Soviet textbooks and the abandonment of traditional interpretations of Soviet history. The Gorbachev leadership itself has been highly critical of conservative ideological orthodoxies and dismissed their relevance on numerous public policy issues, including foreign policy. Finally, as a backwash to the failed coup last August, the communist worldview lost whatever remained of its intellectual authority. Mills could not have anticipated the most recent developments; however, the trend has been there since the introduction of glasnost in 1986.

Why do such views persist? Of what consequence are they? How do they influence policy? Most studies assume policy relevance. "Intercultural misperceptions," argues Mills, have been a serious barrier to improved relations. Reality, I would argue, suggests otherwise. In actual fact, the mindset described by Mills has been of marginal significance. The improvement in U.S.–Soviet relations since 1986 has been shaped more by Gorbachev's anti-ideological "new political thinking" than by an assessment of Presidents Reagan and Bush as "instruments" of the American political–economic elite.

Judgments regarding the relevance of the perceptions apparently still held by the Amerikanisty must be qualified by the fact that they have been rather easily set aside. The author himself notes that the Soviets have been eagerly studying the working of American bourgeois democracy. In their efforts to set up democratic institutions of their own, contingents of Soviet officials have for several years now been visiting the United States in ever-growing numbers seeking guidance as to how to organize their new political structures—legislatures, political parties, public opinion polling, etc. They have inquired about the process of recruiting congressional staffers, the functions of the Library of Congress, the efficacy of U.S. antitrust legislation, effective procedures for legislative monitoring of the defense budget, and so on. Well before the coup, a Soviet security official was quoted in the press as suggesting that his colleagues take part in exchanges with U.S. police agencies to study how to manage mass demonstrations without violence.

However critical Soviet analysts have been in their views of bourgeois political institutions and practices, Moscow is eager to learn from the experience of the United States. Soviet academic analyses of American politics and society, it would seem, have been of marginal consequence to the Gorbachev leadership. Needless to add, orthodox judgments regarding the class essence of American "monopoly capitalism" have become irrelevant in the face of Soviet economic collapse. This was vividly brought home by none other than the erstwhile general Secretary of the CPSU last September in his response to a proposal by Treasury Secretary Brady to create a special training center for "capitalists" in Moscow, whose graduates would be trained to help put the Soviet Union's centralized, planned economy on a free-market basis. According to press accounts, Gorbachev responded enthusiastically to the Brady proposal, offering use of the recently vacated building of the party's Central Committee for this purpose. Clearly, the risk of political domination by the bourgeosie and exploitation of the working class are a small price to pay for an economy that works.

One problem is that Mills takes his source material too literally. The writings he has analyzed more often tell us about the ideological environment that has until recently dominated the Soviet scene than provide authentic intellectual judgements regarding how Moscow sees us. Mills hints at this in his discussion of Aleksandr Yakovlev's *On the Edge of an Abyss* published in 1985. Yakovlev's presentation, which embodies a bitter denunciation of postwar U.S. foreign policy, caused some consternation in U.S. policy circles at the time, given the author's prominence as an advisor to Gorbachev. The book, Mills suggests, may be a "smokescreen." Quite so. Yakovlev may not, in fact, look kindly on the U.S.; but it was apparently to deflect attention from his real political agenda, that is, radical political reform, that he felt obliged to adopt the rhetoric of the most orthodox critics of the United States. With the political collapse of the party theocracy and its legitimizing mythology, we can anticipate a respite from such ideological contortions and, hopefully, a less crippled approach to U.S. politics and society.

U.S. Department of State MORTON SCHWARTZ

Foreign Policy under Carter: Testing Multiple Advocacy Decision Making. By Alexander Moens. Boulder: Westview, 1990. 201p. $29.00 paper.

To those familiar with Alexander George's *multiple advocacy* model of decision making, declaring the Carter administration as a test case may seem an oxymoron. The central role George envisioned for an "honest broker" (preferably the national security adviser) to safeguard an open and balanced policy process contravenes the common interpretation of Carter's national security advisor Zbigniew Brzezinski as a vociferous advocate who sometimes skewed the policy process to favor his own views.

Alexander Moens challenges this conventional wisdom and argues that Brzezinski was in fact an honest broker for the first eighteen months of the Carter administration. In Moens's view, it was only the failure of Secretary of State Vance, Secretary of Defense Brown, and others to offer a wide range of policy options that pushed Brzezinski from broker to advocate in the second half of Carter's presidency.

Moens bases this view on case studies of Carter administration decision making on four issues: "deep cuts" in SALT II in early 1977, the Ogaden War in 1977–78, normalization of U.S. relations with China in 1978, and the fall of the shah of Iran in 1978 and 1979. He argues that these cases trace Brzezinski's evolution from "reasonably neutral management" of SALT, to a precarious combination of advocacy and brokerage on the Ogaden War, and finally to outright advocacy and circumvention of an open policy process on China and Iran. The reason for this transformation, Moens maintains, was that a consensus among top officials presented Carter with an excessively narrow range of options, with which Brzezinski increasingly disagreed. In particular, with no other strong advocate of a harder line toward the Soviet Union, Brzezinski inevitably slipped into an advocacy role that suited his ideology and temperament.

Moens's provocative interpretation is made more persuasive by his effective use of secondary sources and interviews with key officials, including Brzezinski and Brown. Unfortunately, he did not interview Carter or Vance. Thus, his criticisms of Vance and qualified praise for Brzezinski require caution. Moens also lacked the internal documents that would make or break his case. It is unclear, for example, whether Brzezinski's daily intelligence briefing to Carter or his cover memos on materials going to Carter constituted advocacy and compromised his ability to be an honest broker from the start or not.

This book nonetheless provides a useful addition to histories of the Carter administration and the writings of Alexander George, David Hall, and Roger Porter on the multiple advocacy model. Moens reaffirms these authors' views on the tension between the roles of broker and advocate; and he extends them in arguing that even the most honest broker's attempts to bolster underrepresented views will slip into disruptive advocacy. He thus disagrees with suggestions that the broker can rebalance the process by direct advocacy or by bringing in outside advocates. This conclusion may be too categorical, given the constructive role of the "wise men" who advised Johnson to deescalate the Vietnam war, as well as the similar role that members of the Congress, the media, and public interest groups have served in advocating underrepresented views to the president.

To a greater extent than most analysts, Moens presents the multiple advocacy model as the best of all possible worlds, though he is well aware that it can only work if it suits the management styles of the president and his top advisers. He deliberately sets aside the question whether mistaken perceptions led to flawed decisions that no process could have corrected; and apart from a few brief references to the Reagan and Bush administrations, he makes little attempt to update his thesis. In these issues, one could either commend Moens for not getting distracted or criticize him for key omissions. He argues, for example, that it is all but impossible for the national security advisor to be a broker without becoming an advocate, but he does not comment on whether current National Security Advisor Brent Scowcroft has or has not succeeded at this.

Moens's final prescription deserves careful and critical consideration. Because he sees the roles of broker and advocate as incompatible, he recommends designating the national security advisor solely as an advocate and "roving adviser" without primary responsibility for chairing committees, managing crises, or implementing policies. As for the broker role, he calls for reviving the position of executive secretary of the National Security Council. The danger is that a "roving advisor" could become a loose cannon, and relations with the executive secretary could become strained. Would the secretary really reign in the NSC? Would both players have enough status and staff to serve their roles? On these and other issues, Moens provides a needed challenge to accepted views; but until more documents and insiders' details become available on the Carter administration and scholars scrutinize the apparent successes of the Bush administration's decision processes, the jury must remain out.

Georgetown University ANDREW BENNETT

Friends but No Allies: Economic Liberalism and the Law of Nations. By Stephen C. Neff. New York: Columbia University Press, 1990. 345p. $37.50.

This book presents a history of the international law of commerce. Neff builds his narrative around three "experiments" in free trade that produced successively more liberal commercial law: the nineteenth century, the twentieth-century interwar period, and the postwar world. He also provides more detailed discussion of three contemporary topics: international law on economic warfare, economic nationalism in the Third World, and Third World efforts to construct a more "social democratic" international order. The narrative is clear and well written, and Neff connects legal developments to their economic and political contexts. While focusing on legal issues, he presents a good overview of the economic and political developments of each period.

Neff argues that the results of each experiment depended on the struggle between the forces of free trade and those of economic nationalism. He treats these forces as ideas, sets of legal theories. Although he clearly ties these ideas to their political and social contexts, his focus is nevertheless very different from the more common interpretation that these ideas are smokescreens for material interests that gain or lose from free trade. Largely because the analytical approach remains implicit, he never justifies this analytical choice.

One reason he does not justify this choice is that his approach is atheoretical. Although familiar with the theoretical literature in political science and related fields, he chooses not to contribute to it. In a book spanning several centuries, this leaves much of the narrative hanging in air, without clear causal analysis. For instance, Neff stresses the personal influence of Cordell Hull over American trade policy in the 1930s and 1940s. Without denying Hull's role, I would have liked Neff to explain how a man such as Hull comes to power and remains there: Is it Hull's genius, or does he reflect a certain economic interest or political coalition? Neff's disinterest in theory and relative inattention to causal analysis means that we do not learn the answers to such questions.

His lack of theory is also reflected in a sometimes weak knowledge of the economic issues involved in the subjects he studies. For instance, he makes the repeated claim that nonindustrial countries benefit less (or perhaps not at all) from international trade. Clearly, this flies in the face of the classical law of comparative advantage. While there exist theoretical positions Neff might use to make his case, whether contemporary strategic trade theory or traditional dependency theory, Neff does not provide even a preliminary justification for his position.

This is problematic because Neff's normative position would appear to be close to that of the classical liberals whose positive theory he is unwilling to accept in its entirety. In fact, the book would be stronger throughout if Neff had presented his normative position explicitly. He clearly sides with the liberals, but we never find out why.

Its limits notwithstanding, *Friends but Not Allies* will familiarize political scientists to a legal perspective on the international political economy that they may not frequently see. Neff writes at a level appropriate for advanced undergraduates, and the book would be useful for courses desiring a legal angle on international economic relations.

University of Rochester ROBERT PAHRE

War, Peace, Survival: Global Politics and Conceptual Synthesis. By Robert C. North. Boulder: Westview, 1990. 298p. $48.50 cloth, $17.95 paper.

What causes war? In his past work Robert North (and Nazli Choucri) have typically identified "lateral pressure" as the cause of imperial and territorial expansion among pre–World War I European states. In that period—and perhaps even today—a country's "lateral pressure" upon other states is apparently directly correlated with its power. This does not mean, however, that nations expand wherever and whenever their power permits. Uncertainty and the "power paradox" (the greater power a nation acquires, the more insecure it may often become) may dampen such outcomes. Still, North's views are like those of the "power transition" school in that intersections (crossing points, collisions) of interest and power with other states may well cause war.

The present work, however, modifies and extends North's previous findings. In particular he contends that beta countries are likely to be expansionist. This conclusion requires some explanation. North breaks countries down into categories, reflecting their population and economic growth, technology, per capita wealth, territory, and resources. Alpha countries are those which possess high technology, a relatively stable population, large stocks of resources, a high per capita gross national product, and a relatively large territory, such as the United States and (until recently) the Soviet Union. Beta countries, in contrast, are characterized by population pressure (large population relative to the size of the territory), increasing technology but inadequate resources relative to population, and a high per capita gross national product. North lists Great Britain, France, and the Netherlands as countries that were in the beta stage when they undertook their overseas colonial expansion; Germany and Japan assumed a beta mantle before World War I and again prior to World War II. North writes, "A society with this profile is under continuous pressure to expand its trade, or if that recourse is blocked in one way or another, to expand its territory" (p. 127). Gamma countries are those like contemporary Japan, Germany, France, and Britain, that is, high-technology countries that offset any modest population pressure by a growth in technology, economics, and trade. Delta nations are exemplified by the Nordic and small countries of Western Europe, which face no population pressures and have ample technology. Epsilon countries are typical population-pressed LDCs like Bangladesh, India, and China. They are positioned to be challengers like betas but are too poor (low per capita gross national product) to go to war. Zetas are nations with little population pressure (and therefore no temptation to expand) and are very poor (e.g., Chad and Bolivia). Finally Etas are countries with little population pressure, developing technology, high resources, and a high per capita gross national product. The oil countries of the Middle East are representative of this category.

If we take this depiction seriously, a theory of war is clearly implied. The countries that would be expected to be most peaceful would be the delta group, in which population pressure is low or nonexistent, technology scarcely needs to compensate for it, and trade is at a high level, providing an alternative outlet for any other expansionist demands. From North's point of view, it is perhaps not surprising that these are the states identified by John Mueller as early converts to the peaceful camp. Gamma countries are somewhere between deltas and betas: they house a large population within a confined territory and only sublimate their potentially expansionist drives through foreign trade and high-technology growth inside the state. Any major economic setback, such as domestic depression or a major cut in international trade, would pose questions for their peaceful orientations. Epsilon countries will also become potential expanders when their growing wealth makes this possible. The challenge for the world is to neutralize this tendency through further domestic high-technology growth and expanding trade. Another interesting implication is that foreign countries may wish to reduce population pressure in epsilon nations by taking immigrants from them, as the United States has done in recent years. In the nineteenth century—perhaps from this point of view—the then population-pressed European states exported their excess citizenry to the open spaces of the New World. In part, North is aware of these implications and certainly recognizes that world population growth presents a formidable problem when it is not accompanied by offsetting technological development. He does not, however, seize upon the many policy-relevant implications of his analysis.

Is the analysis correct? He may have identified one strand of theory that seeks to explain conflict, but he misses many others. The realm of ideas and ruling intellectual conventions that contribute to either peace or war are left unexplored. But national metaphors are potent forces. The idea of liberalization and democracy is now moving to the fore and is having a great effect, even upon the frequency of war (see the work of Michael Doyle). Nations that conceive their role to be that of trading states behave differently from those of a military orientation. In the past, military and biological metaphors foisted erroneous "expand or die" notions upon late-nineteenth-century European states. North also misses the stabilizing role of nuclear weapons as a restraint upon conflict. He seems to think that weapons of mass destruction only exacerbate frictions among nations.

Finally, his analysis is ultimately inconsistent in theory itself. If the factors that make for war and expansion ("lateral pressure") are physical and economic ones, it is difficult to believe that new conflict resolution techniques can deal effectively with them. Only a cessation of those pressures permits peace and survival. In the final section of his work, North introduces a fourth, or global, level of international interaction. Instead of fruitfully developing this, however, he unfortunately goes on to make recourse to many of the same factors that he considered under the third, or international, level.

Ultimately, the work contains some intriguing ideas that might have been carried further in specific cases but disappoints in its overall theoretical presentation.

University of California, Los Angeles RICHARD ROSECRANCE

Who's at the Helm? Lessons of Lebanon. By Raymond Tanter. Boulder: Westview, 1990. 262p. $24.95.

Raymond Tanter, an experienced academic observer of Middle East affairs, has now written an insider's account of U.S. foreign policy decision making during the Lebanon crisis of 1981–83. A professor of political science at the University of Michigan and senior staff member on the National Security Council from 1981 to 1982, he benefits from his perspective as an academic, as well as a participant in the policy process. His study exhibits both the strengths and weaknesses—the insights, as well as the strains—produced by that mingled perspective.

Tanter sets out to answer three main questions: Which officials should regulate political and military affairs? Why do leaders make decisions to combine or not to combine political and military actions? What is the impact of crisis on process and choice? (p. 2). His focus is on interagency politics, on the struggle for control of Mideast policy in a Reagan White House depicted (in a nautical metaphor that pervades the book) as "adrift." Because the president preferred a hands-off policy management style, we are told, no one was "at the helm"—or rather, there was a bitter struggle over who would take the helm between *globalists* and *regionalists* (pp. 19–23). Indeed, there is a strong impression that matters can be simplified even further, to the Manichaean competition between two men: globalist Secretary of State Alexander Haig and regionalist Deputy National Security Advisor Robert McFarlane, representing good and evil, respectively. McFarlane comes in for sustained and severe criticism for mismanaging "the intelligence and policy process for his own bureaucratic ends and to the detriment of national security" (p. 4).

Thus, the answer to the question who should regulate political and military affairs is that the president and his principal foreign policy advisors, the secretaries of state and defense, should. This is sensible enough, but his suggestion that the national security advisor "should operate as an honest broker not as an independent policy formulator" (p. 226) is problematic at best. That office has been a major locus of foreign policy formulation since it was occupied by Henry Kissinger. Moreover, the heavy criticism of McFarlane's performance may, with cause, be dismissed by some as merely a judgment on personality, not of the office or its role in the policy process per se.

Tanter's observations of real decisions are often keen and insightful and advance understanding of the question about combining political and military actions at least in the specific context of the Lebanon crisis. But his overall conclusion—that presidents should strive to be more aware of "how difficult it is to combine diplomacy and force" (p. 226)—while unarguable, is also no more than a truism. The answer to the question on the impact of crisis is also his best-demonstrated general conclusion. As a result of Reagan's failure to lead his policy team, Tanter argues, strategic opportunities were missed vis-à-vis both the Soviet Union and the Arab–

Israeli conflict. No advantage was taken of Israel's invasion of Lebanon to enhance regional security or push back the reach of Soviet influence. Tanter notes (agreeing with other literature on crisis decision making) that a primary effect of a crisis on the decision-making process is to permit the White House to lead in a way not normally possible. (There is less low-level bureaucratic resistance to presidential leadership in a crisis.) His criticism of Reagan's failure to seize the opportunities presented by Lebanon thus carries considerable weight. And he goes further, asserting that Lebanon was a precursor to the Iran–Contra affair. As he puts it, "If Lebanon represents a misuse of the interagency process, then the Iran episode indicates a disuse of that process. . . . The legacy of Lebanon is Iran" (p. 2).

Readers familiar with the by-now-significant body of literature on the U.S. foreign policy decision-making process will find much that is familiar in this book; but beyond yet another "case study" example, they will not find much that is new. Specialists in Mideast affairs, on the other hand, should welcome the book as a valuable compendium (much of it based on insider interviews and personal observation) of the Reagan administration's actions and decisions during the Lebanon crisis. That should hold true despite the book's opaque prose style and occasional flights of purple fancy, like "[Secretary Haig] had outfitted the entire sixth floor of State in his ideological attire. But, upon [his] abrupt departure, they shed this global garb in favor of tighter fitting, regional raiment" (p. 36). Overall, the book will be of use to specialists, since it is rich in detail and anecdote if at times cluttered, as well.

University of British Columbia CATHAL J. NOLAN

Reserve Forces and the British Territorial Army: A Case Study for NATO in the 1990s. By Wallace Earl Walker. London: Tri-Service, 1990. 190p. $45.00 cloth, $20.00 paper.

Civilian-based Defense: A Post-military Weapons System. By Gene Sharp. Princeton: Princeton University Press, 1990. 152p. $19.95.

The effective end of the cold war and its military edifice in the form of the NATO–Warsaw Pact military confrontation in Europe has had numerous effects on both the international system and the domestic politics of the former competitors. With the rationale for large, standing forces-in-being in Europe to blunt a Soviet surprise invasion of Germany now effectively dissolved by events, the size and shape of standing military forces in the future has become a lively topic of debate.

It is a debate that encompasses almost all the NATO countries. The economic burden of maintaining large active forces has created considerable pressure to reduce the size and expense of forces in the future to allow the redirection of resources to other priorities. The question—hotly debated both within and outside the services—is how to reduce forces without critically compromising the ability to ensure the national security of member states of the alliance and beyond.

These two books stand at opposite ends of the periphery of that debate. The Walker book, a case study of the experience of the British Territorial Army—Britain's primary reserve force—attempts to extrapolate the British experience to other NATO members. Its primary purpose is to raise, and attempt to answer, the question, How much and what kind of role can reserve forces play in the future? Since active forces are diminished in size

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and the temptation to rely more on the reserve component builds in a number of countries (most prominently, the United States), it is a germane question.

The Sharp book stands in rather vivid contrast at the far end of the spectrum. More a polemic and advocacy than a work of scholarship in the traditional sense, it argues for a process that would eventually eliminate military forces and replace them with a system of "civilian-based" action where defense against military aggression would be accomplished through such peaceful means as passive resistance.

In the short run of advising policymakers, the Walker volume is clearly the more useful. The recently concluded Desert Storm operation, in which American reserve forces were mobilized and committed for the first time since the Korean War, has raised some serious organizational, military, and political questions that must be answered for a future in which reductions in active component forces means that any large operation will require an enhanced level of reserve participation.

The British experience with the Territorial Army is parallel and instructive to the American. The Territorial Army has been looked down upon by active forces, has been involved in political wrangles over role, mission, and even levels of funding, equipment, and training, and these are all themes that recurred during Desert Storm. The comparative lessons of the Americans and the Territorial Army may prove of some value for dealing with the future.

The Walker book can be read on two distinct levels. First, it can be read as a study on the problems of reserve forces and their uses in war. Walker, in this regard, provides interesting and provocative insights into such practical problems as the deployment and mobilization of reserves and their contribution to the overall combat power of an armed force. In this respect, he challenges some assumptions made within the American military establishment. Second, the book can be read as a study of organizational behavior, including a strong discussion of bureaucratic and grassroots politics. It is hard to disagree with Walker's conclusion that the reserve forces issue is "a rich and unexplored territory" for future study.

The Sharp book is totally unlike the Walker volume. Undoubtedly influenced heavily by Richard Falk's view of conflict and war, Sharp sets out to explore, and provide a framework within which to implement, his idea of civilian-based defense. The core is that ordinary citizens, by simple acts of nonviolent resistance, can act as an effective alternative to traditional military sources. He cites numerous instances in the past of how this has been accomplished; and he would, I suspect, argue that the successful resistance of the Soviet population to the August 1991 attempted coup represents the kind of action he advocates.

The weapons that Sharp argues can be effective are not traditional arms but, instead, psychological, political, and economic weapons, achieved through a process of what he calls *transarmament*.

Sharp is entirely up front in his assertions. In the preface he states the two premises on which his thesis resides: "that civilian-based defense policies against internal takeovers and foreign aggressions can be developed, and that dictatorships and oppression can be prevented and disintegrated by the capacity to wage powerful nonviolent struggle." If one accepts, or is at least receptive to, these premises, Sharp's argument and plan may be convincing. If one is skeptical of the premises, however, there is little here that will cause a conversion.

University of Alabama DONALD M. SNOW

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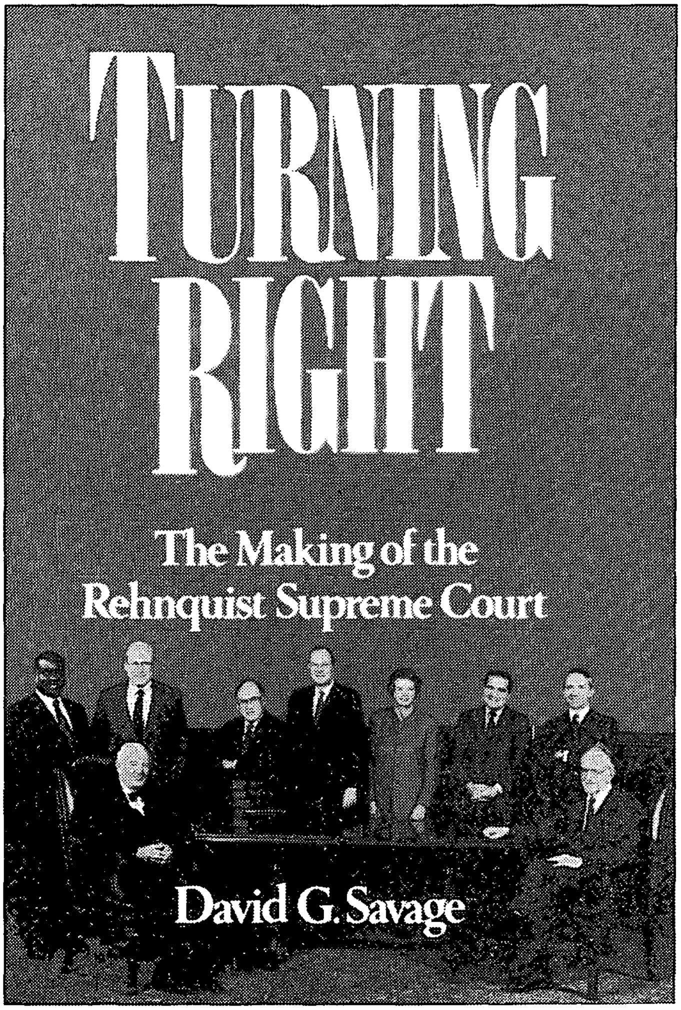

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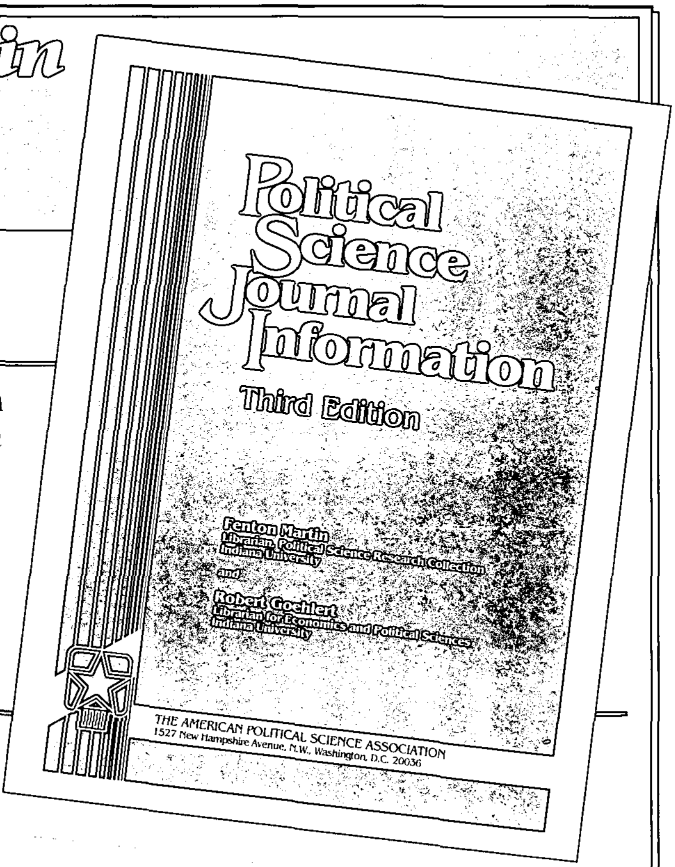

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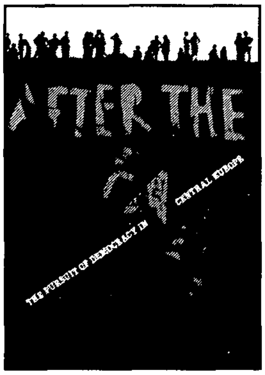

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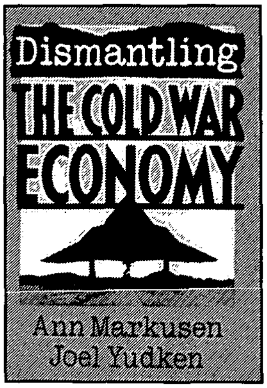

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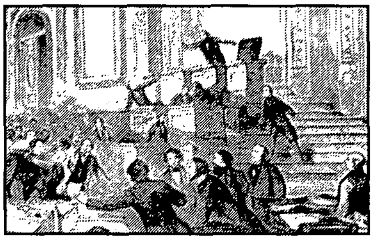

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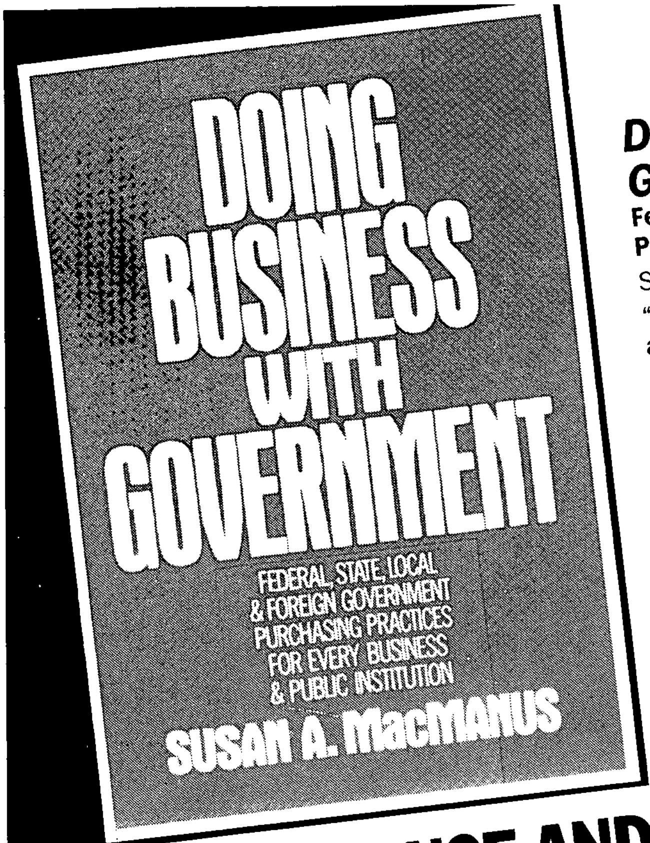
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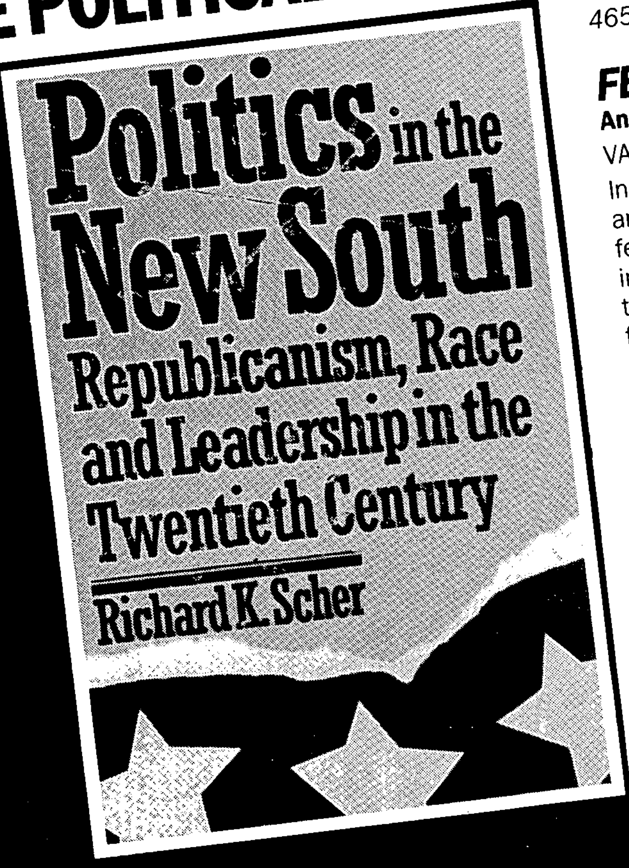
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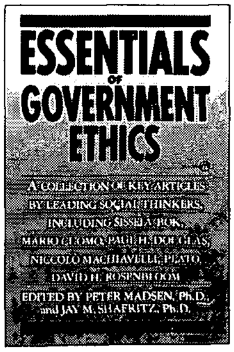
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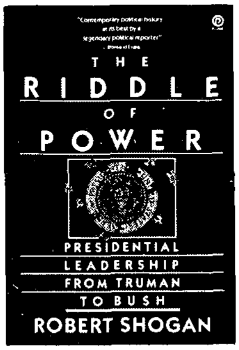

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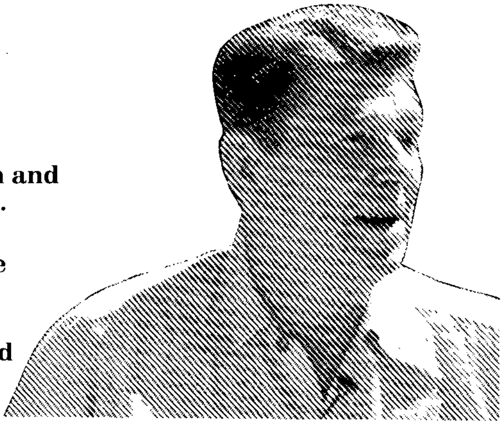

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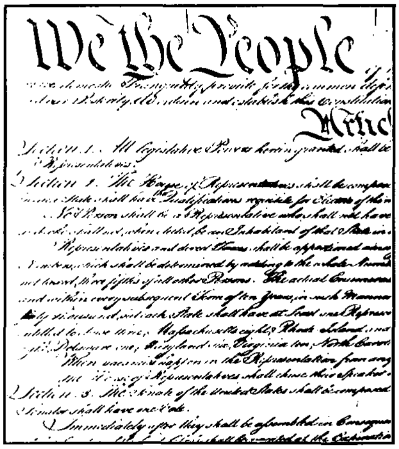

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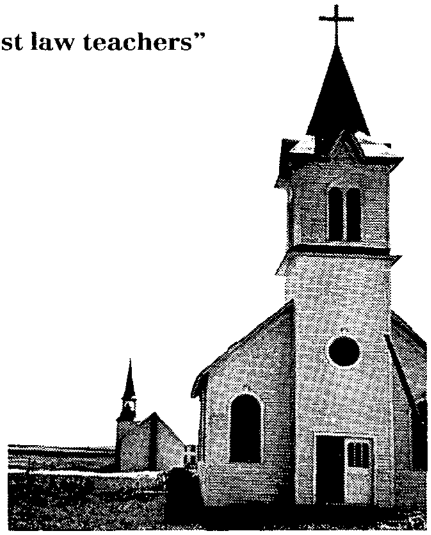

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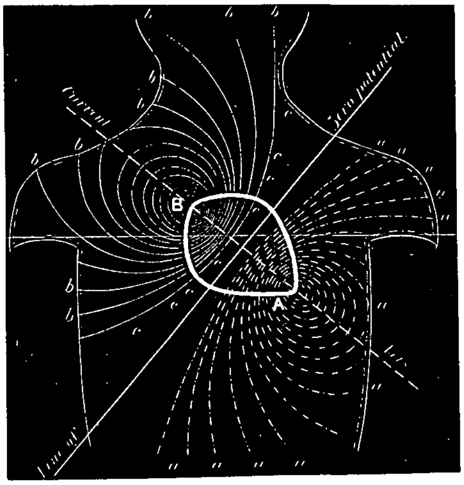

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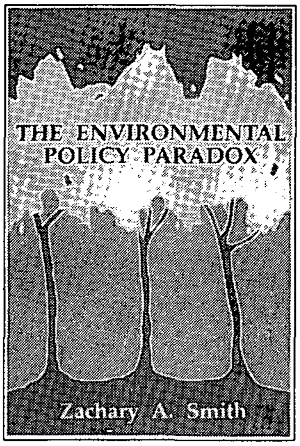

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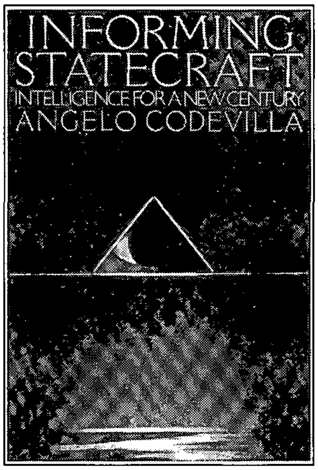

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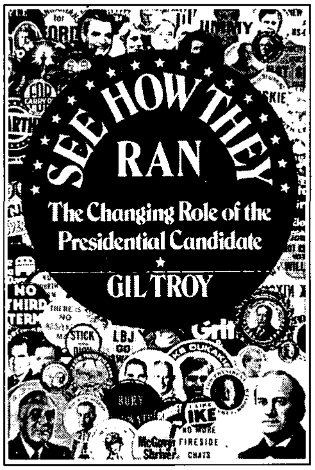

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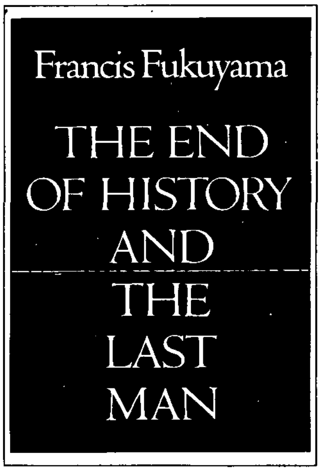

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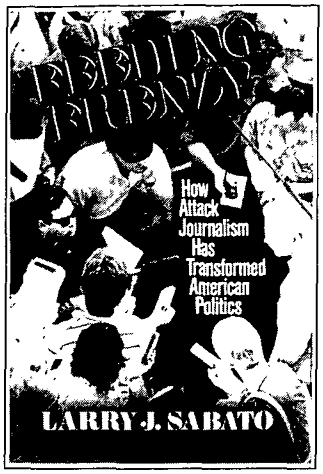

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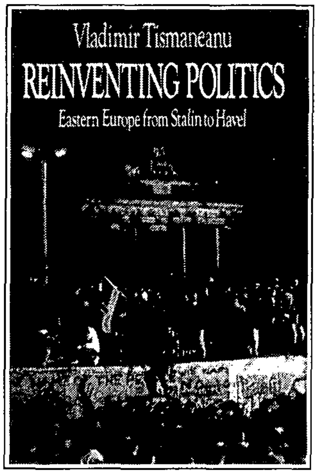

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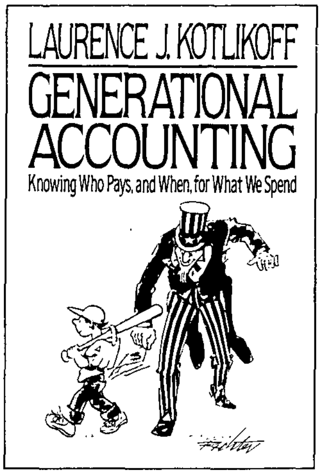

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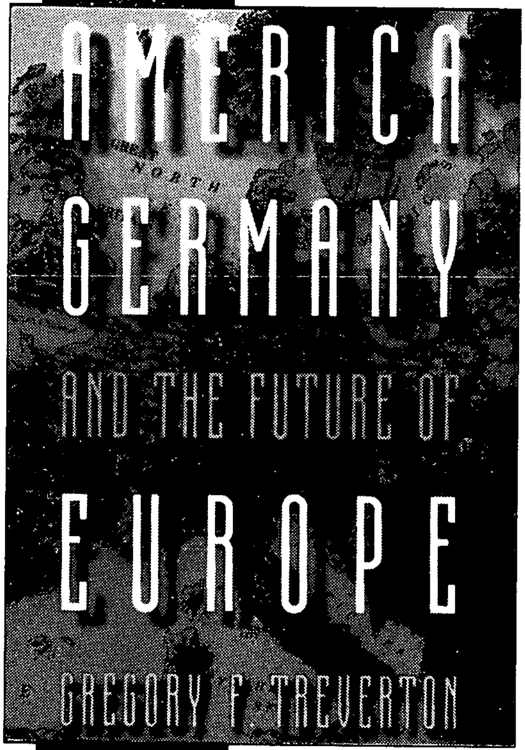

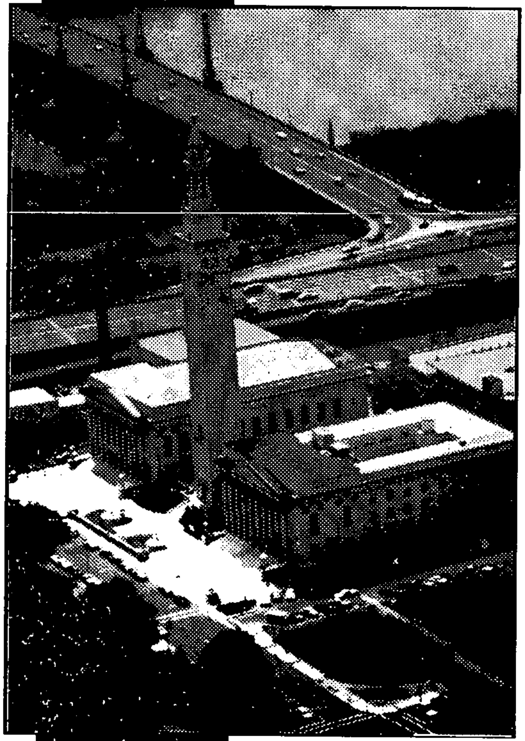

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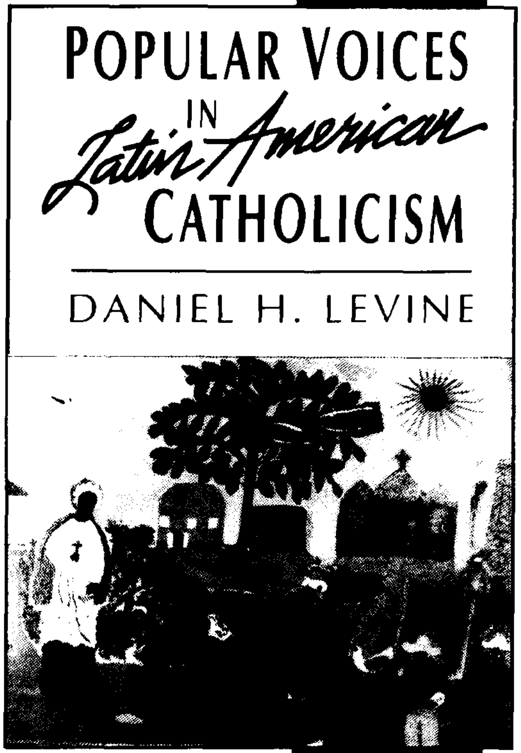

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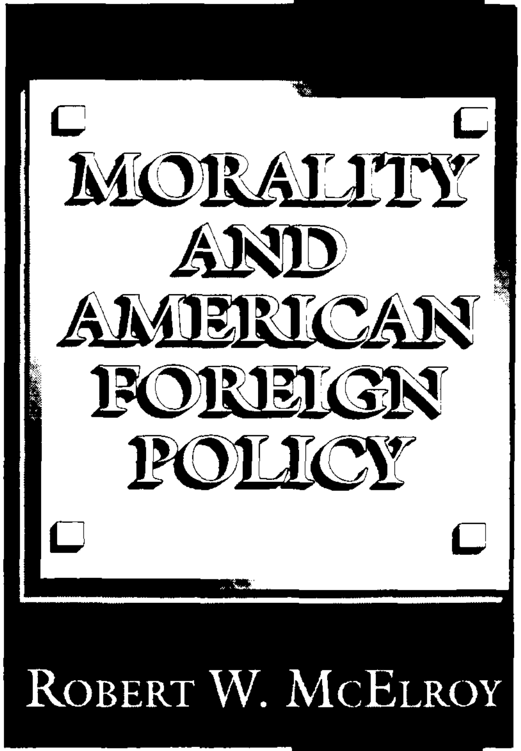

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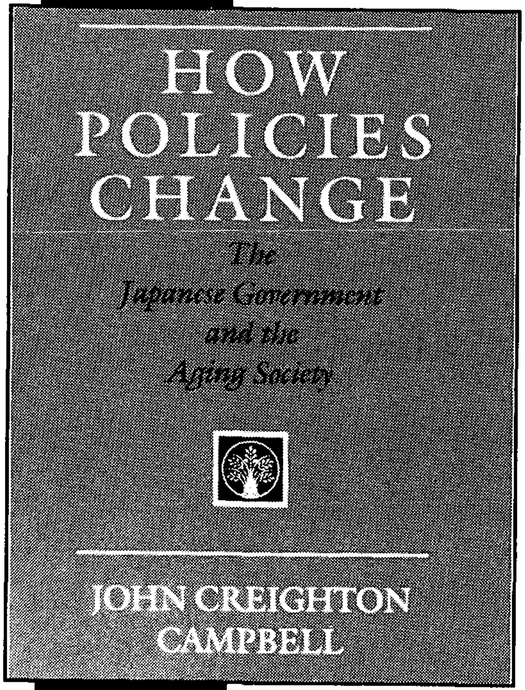

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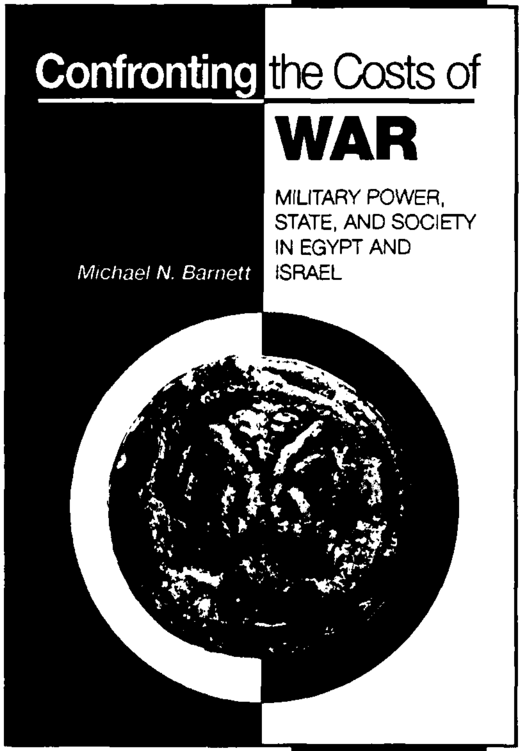

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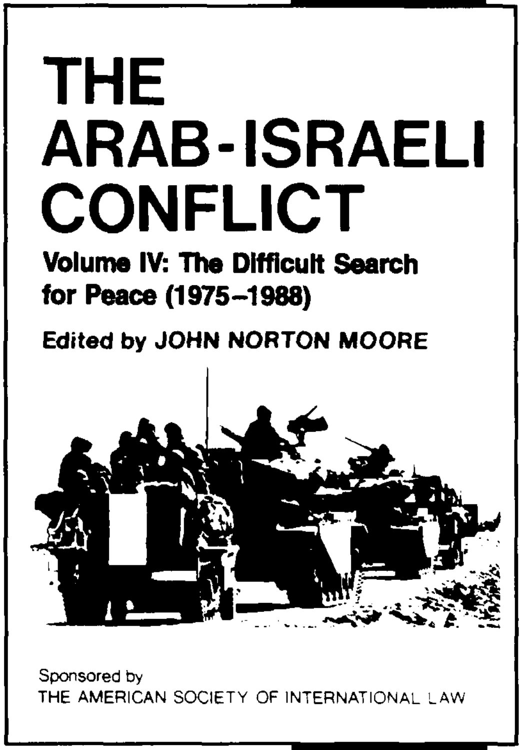

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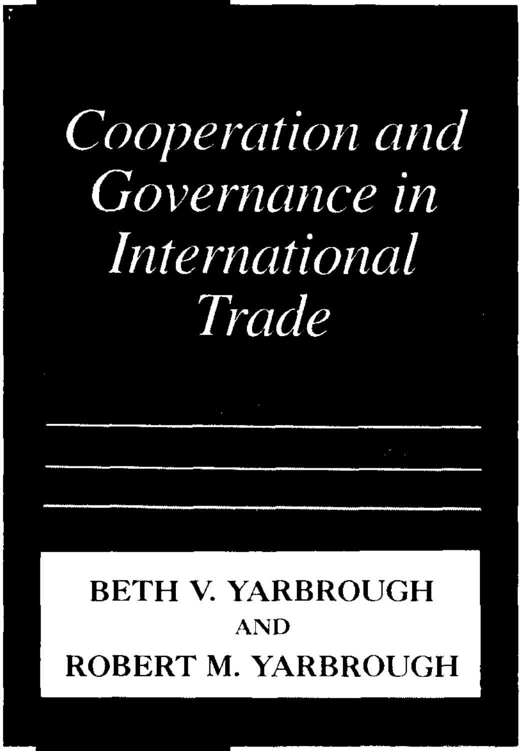

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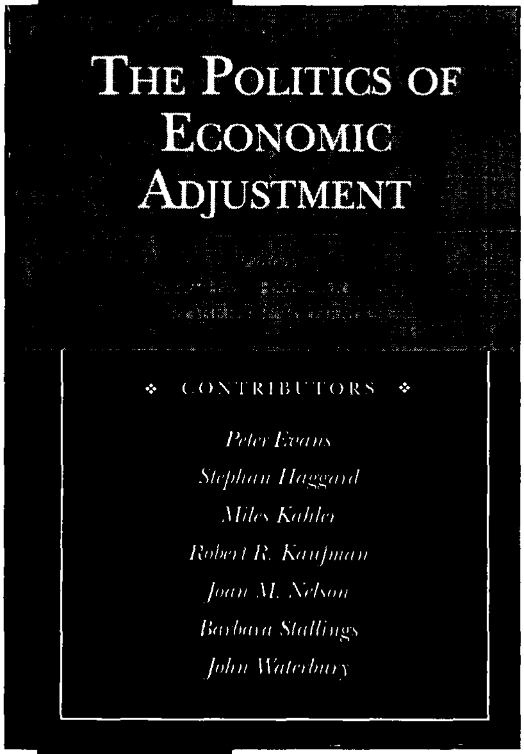

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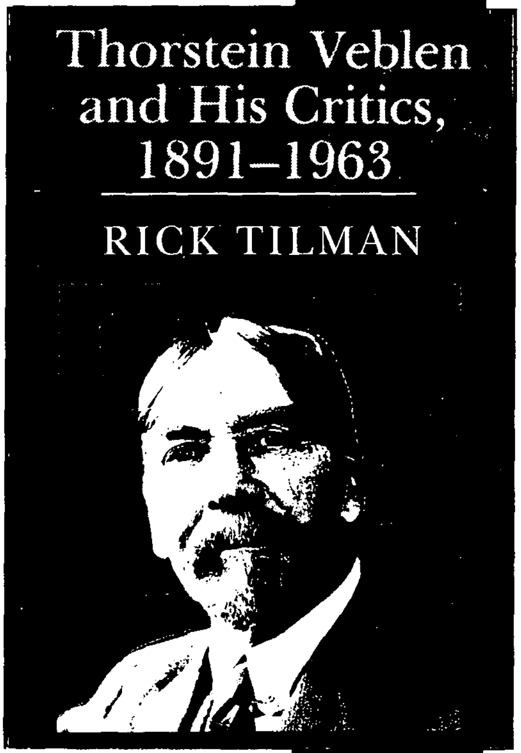

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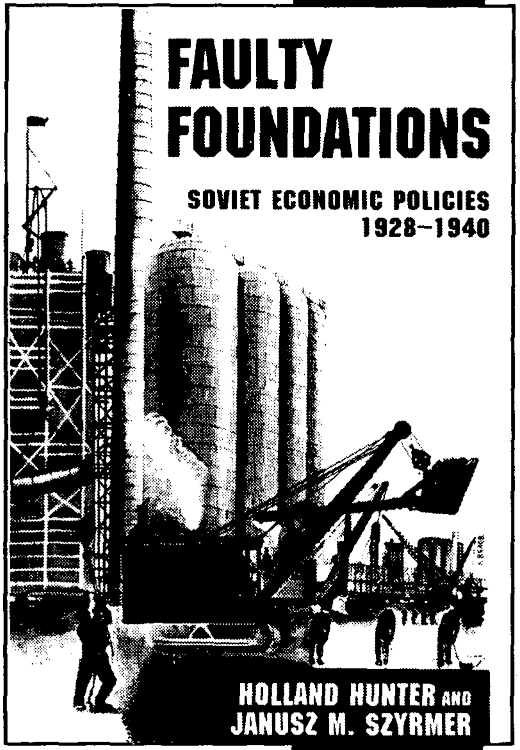
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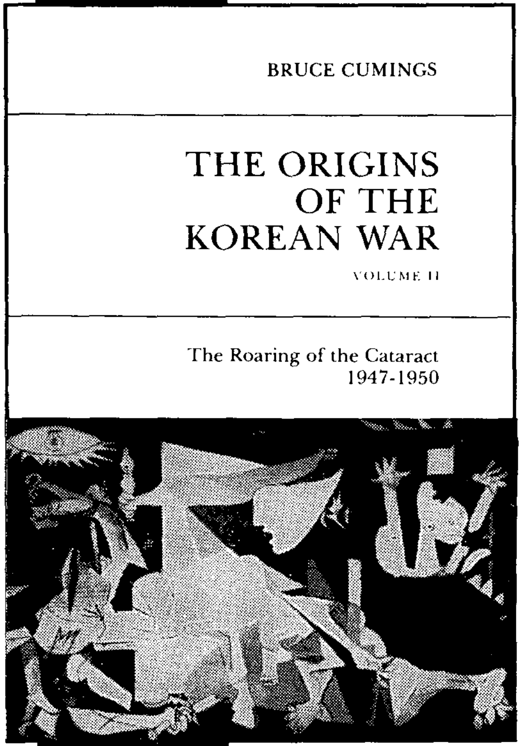

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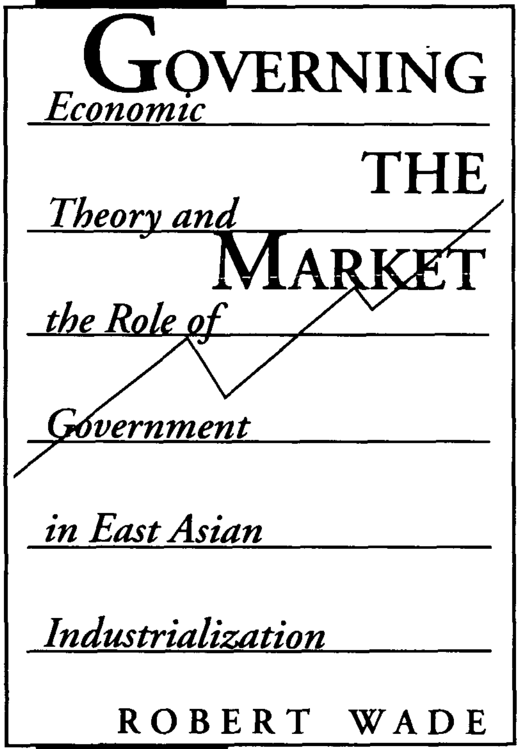

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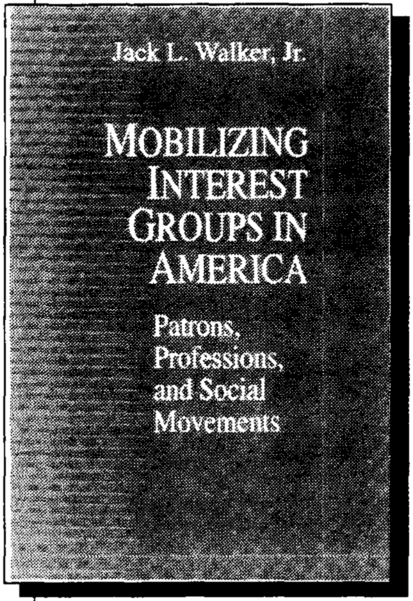

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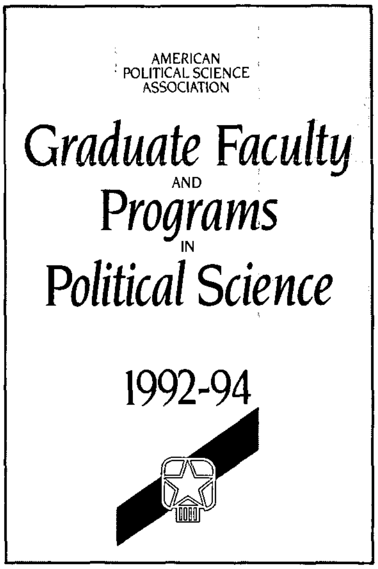
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